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ABSTRACT

The report assesses the effectiveness of STETS, the Structured Training and Employment Transitional Services, a project to prepare mentally retarded young adults for competitive employment. An initial section provides an overview on transitional employment, including sections on public policy and transitional employment and related services. The STETS demonstration is described in terms of its model, underlying research issues, and the data collection process. Implementation of the model is the focus of chapter 3 which examines the sample selection process, the response rate to surveys, the quality of data, and the use of analytic methodologies (impact analyses and benefit-cost analyses). The succeeding six chapters document STETS impact on the following aspects: (1) employment, earnings, and other labor market outcomes; (2) training and schooling; (3) public transfer dependence; (4) economic status, independence, and life-style; (5) benefits and costs; and (6) participants' outputs. Among key findings are that STETS improved the postprogram employment prospects of mentally retarded young adults, that participants benefitted substantially from the program, and that STETS created intangible benefits by increasing available employment and social opportunities. Policy conclusions are noted regarding program potential to mitigate the independence and employment problems of mentally retarded young adults, program design and targeting, and program benefits and costs. Six pages of references, supplementary tables, survey design and implementation, and benefit-cost analysis methods and results are appended. (CL)

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IMPACTS OF TRANSITIONAL EMPLOYMENT FOR MENTALLY RETARDED YOUNG ADULTS: RESULTS OF THE STETS DEMONSTRATION

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Mathematica Policy
Research, Inc.

Manpower Demonstration
Research Corporation

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The evaluation effort underlying this report was necessarily long and complex. The project benefitted greatly from retaining an excellent core project team throughout its life. Susan Stephens designed most of the data collection documents, co-directed the survey implementation, and directed all phases of the data collection effort after the start-up phase. She also contributed substantially to the research effort itself. Anne Bloomenthal managed the project from its inception. This managerial role involved working on every aspect of the evaluation, including database development, programming, research, and financial management, and generally catching every loose end that can develop in such a project. She provided a much needed continuity to the effort and performed efficiently and professionally. Expert advice and review was provided by Robert Bruininks from the University of Minnesota. He gave totally unselfishly of his time and expertise, and taught us a great deal about the subject of our research.

Many other staff members contributed to the study design. John Burghardt, Walter Corson, and Rebecca Maynard conducted a program and evaluation concept study that guided the implementation of both the program and the evaluation. Rebecca Maynard also played an important role in the final analytical effort. Russell Jackson, as the survey director for the design and implementation phases, developed the data collection procedures, and generally set the data collection effort on a path that maximized its accomplishments. As consultants to the design effort, Carol Sigelman from the Eastern Kentucky University, Frank Rusch from the University of Illinois, and Laird Heal from the University of Illinois provided expert advice on data collection and research issues.

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Finally, this study was made possible by the cooperation and persistence of the young men and women who participated as experimental and control group members. It is our hope that the results of this study will make a contribution toward improving the opportunities available to others who must overcome similar obstacles.

Stuart Kerachsky
Craig Thornton

Principal Investigators
Mathematica Policy Research, Inc.

PREFACE

This report and the publication of a related monograph, Lessons on Transitional Employment, mark the culmination of the STETS demonstration. STETS was a major experiment testing the feasibility, cost and effectiveness of a transitional employment program for mentally retarded young adults. Starting in 1981, the Manpower Demonstration Research Corporation (MDRC), the nonprofit corporation that managed the demonstration, traced the experiences of five diverse organizations that aimed to prepare mentally retarded youths between the ages of 18 and 24 for competitive employment. The program drew on MDRC's National Supported Work Demonstration -- a structured work experience program shown to be successful in helping long-term welfare recipients obtain regular employment. Like Supported Work, STETS tried to acclimate participants to the regular work environment in gradual stages over about a year's time.

In a 1984 implementation report, MDRC concluded that although the challenge was one of considerable magnitude, the program could be feasibly operated by a variety of agencies and could help many mentally retarded citizens make the transition to competitive jobs. At the same time, the study stressed that not all participants could be placed in competitive jobs and that alternative programs and services would still be needed.

At that point, however, the most important question -- whether STETS was actually effective -- was still unanswered. To learn whether or not participants would have fared just as well without this intervention, it was necessary to compare outcomes for them and a matched control group, who, although they could be included in other programs, were not offered

STETS services.

Such a comparison falls within the purview of the STETS impact analysis, which MDRC subcontracted to Mathematica Policy Research, Inc., and which is the subject of this report. This report presents findings for a group of over 400 experimentals and matched control group members at 6, 15 and 22 months after enrollment. It also contains a benefit-cost analysis.

The study offers convincing evidence that STETS is a promising approach. Although it is clear that transitional employment should be only one option within the mix of services available to the mentally retarded population, its importance can be gauged by the report's findings: About one year after leaving the program, participants were substantially more likely than their control group counterparts to be working in competitive jobs and less likely to be in sheltered workshops. The program also seemed to be particularly effective for some groups who may have special difficulty finding jobs on their own -- for example, those in the moderately retarded category.

Overall, STETS appears to be an effective investment of public resources. Economic benefits to society seem likely to exceed program costs within three years of enrollment. These findings, along with others from this study, should therefore hold considerable interest for those who have long sought more knowledge on whether mentally retarded citizens can be helped to move into the regular labor market.

Barbara B. Blum
President

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EXECUTIVE SUMMARY

The Structured Training and Employment Transitional Services (STETS) demonstration was designed to provide the first rigorous test of the effectiveness of transitional-employment programs in integrating mentally retarded young adults into the economic and social mainstream. Under the demonstration, which was funded by the Employment and Training Administration of the U.S. Department of Labor and directed by the Manpower Demonstration Research Corporation, programs were operated from the fall of 1981 through December 1983 in five cities throughout the country. This demonstration has greatly expanded our knowledge about the implementation and operation of transitional-employment programs for this target population and has documented the effectiveness of such programs in enhancing the economic and social independence of mentally retarded young adults. Key findings from the evaluation effort include the following:

1. The program did achieve its central objective of moving mentally retarded young adults into regular, unsubsidized employment. It was much more effective during the "steady-state" period of program operations than during periods of start-up or phase-down, suggesting that ongoing programs would have even more favorable outcomes than did this short-run demonstration.

2. The program was not effective in increasing overall employment activity among mentally retarded young adults, but it did substantially increase the probability that they held regular jobs instead of workshop or activity-center jobs; thus, their earnings increased substantially, both in absolute terms and relative to the earnings gains estimated for employment programs targeted toward other disadvantaged subgroups of the population.

3. The program tended to have a greater net impact on the regular job-holding of those whose IQ scores were in the mild to moderate range than for those whose scores were in the borderline range, suggesting that programs should not "cream" from among the applicants they judge to be the most capable.

4. The program tended to be less effective with the women it served than with the men, especially during periods of operational transitions.

5. The STETS model of transitional employment services can be implemented effectively at a seemingly reasonable operating cost relative to other program options and relative to the program's benefits.

This report on the impact evaluation and the benefit-cost analysis of the demonstration program consists of the following components: (1) a brief description of the rationale for the demonstration, and overviews of the STETS demonstration and the evaluation design, (2) a discussion of the success of the program in achieving its impact goals, (3) a comparison of the benefits and costs of the program, and (4) a review of the policy implications of the demonstration findings. A complementary report prepared by MDRC (Riccio and Price, 1984) discusses issues pertaining to program implementation and its potential replication.

THE RATIONALE FOR THE DEMONSTRATION

Transitional employment programs for mentally retarded persons have a relatively brief history, dating back only to the early 1970s, and none of the previous efforts has been subjected to as rigorous an evaluation as this one. Several related factors were especially influential in the initiative taken by the Manpower Demonstration Research Corporation to design the STETS demonstration.

First, over the past 15 years, attitudes have changed considerably regarding the rights and abilities of mentally retarded and other handicapped persons to participate more fully in society and to make substantial contributions to their own support. Among the prominent evidence of this shift are the Rehabilitation Act of 1973; provisions in the Vocational Education Act, the Comprehensive Employment and Training Act, and the Job Training Partnership Act that encouraged the participation of handicapped individuals in education and training programs; the Education for All Handicapped Children Act of 1975; and the 1980 and 1984 amendments to the Social Security Act whose purposes were to reduce the work disincentives created by the Social Security Disability Insurance and Supplemental Security Income programs.

A second relevant factor is that, despite these federal efforts, a small proportion of persons who report a handicap are employed.[1] Moreover, an even smaller proportion of the mentally retarded young adults are employed in regular, unsubsidized jobs. These persistently low employment rates, together with the substantial federal outlays for income support and special education services to mentally retarded persons,[2] have fostered a growing emphasis on intervention strategies, including transitional and supported employment.[3]

A third factor that fostered this demonstration was that two independent bodies of evidence suggested that transitional employment was a potentially effective way to facilitate the transition of many mentally retarded young adults from school or workshop/activity centers into regular competitive employment. First, the results of the national Supported Work demonstration showed quite clearly that transitional employment programs could be effective in mitigating the employment problems of other seriously disadvantaged subgroups of the population, and that the effectiveness of the programs tended to be greater among the more disadvantaged subgroups of the target populations served.[4] Second, a number of relatively small transitional employment programs for mentally retarded adults, many of whom were young, have demonstrated the operational success of such efforts.[5]

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[1] For example, according to the U.S. Bureau of the Census (1982), only about 25 percent of all persons reporting a handicap are employed.

[2] In 1982, approximately 500,000 mentally retarded persons received SSI benefits totalling about $1.5-billion (Social Security Bulletin, 1983); an estimated $225-million dollars were spent on education for 18- to 22-year-olds under P.L. 94-142.

[3] See Bellamy and Melia (1984) for a discussion on the general rationale for the spiraling interest in these interventions.

[4] See, for example, Hollister, Kemper, and Maynard (1984).

[5] See, for example, Rusch (1980), Wehman (1981), Hill et al. (1985), Bailis et al. (1984), Vera Institute of Justice (1983), and Hill and Wehman (1983).

THE STETS DEMONSTRATION

The STETS demonstration design reflects the influence of the national Supported Work demonstration, which emphasized transitional employment under close supervision (with peer-group support) and gradual increases in productivity demands. However, elements of the transitional and supported employment models that have evolved in the disability program arena have also been blended into the program model.

The Target Population and Program Model

The STETS program model was designed specifically to serve the needs of 18- to 24-year-olds whose IQ scores ranged between 40 and 80, who had no work-disabling secondary handicaps, and who had limited prior work experience. It encompassed three phases of activity. Phase 1 consisted of initial training and support services that were generally provided in a low-stress work environment and which could include up to 500 hours of paid employment. Phase 2 was essentially a period of on-the-job training (subsidized or unsubsidized) in local firms and agencies. This period of graduated stress was to promote job performance that matched the performance of nondisabled workers in the same types of jobs. Together, Phases 1 and 2 were to last no longer than 12 months of active time for any participant. By design, Phase 2 jobs were to roll over into postprogram jobs with the withdrawal of program support. Finally, Phase 3 consisted of follow-up services to workers who transitioned into unsubsidized, competitive employment.

Participants and Program Services

STETS was implemented in five sites--Cincinnati, Ohio; Los Angeles, California; New York, New York; St. Paul, Minnesota; and Tucson, Arizona--which were chosen both for their diversity in terms of program sponsors and geographical locations and for their project capabilities. In total, these five programs enrolled 284 participants between November 1981 and December 1982; of this total, 58 percent of our evaluation sample were male, and half were from minority ethnic/racial groups. The majority (60 percent) had IQ scores that indicated mild retardation, and another 12 percent had IQ scores that indicated moderate retardation.

As was expected, the young adults who enrolled in the program exhibited a substantial degree of dependence on others. About 80 percent of them lived with their parents, and another 10 percent lived in supervised settings. Less than 30 percent were able to manage their own finances. Nearly two-thirds were receiving some form of public assistance; one-third were receiving either Supplemental Security Income (SSI) or Social Security Disability Insurance (SSDI).

Prior vocational experience was limited primarily to workshop and activity centers, and about one-third had had no type of work experience in the two years prior to enrollment. However, this group was obviously interested in making the transition to unsubsidized employment, as evidenced by the fact that 70 percent of the participants had worked or participated in an education or training program in the six months prior to applying to STETS.

On average, young adults in our evaluation sample were enrolled in STETS for nearly 11 months, during which period they worked an average of 710 hours in paid employment (370 hours in Phase 1 activities and 340 hours in Phase 2 activities).[1] Participants in New York and Cincinnati had higher than average probabilities of entering Phase 2 employment and tended to have longer-than-average periods of paid employment. Also noteworthy is the fact that a disproportionate share of males entered Phase 2 (73 percent of males as compared with 57 percent of females). Forty-four percent of the program participants (51 percent of the males and 33 percent of the females) transitioned from the program into unsubsidized jobs (primarily in the for-profit sector) that paid, on average, about 10 percent above the minimum wage.

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[1] Those with Phase 1 paid employment (92 percent of all participants) worked an average of 400 hours in their Phase 1 jobs; those with Phase 2 paid employment (66 percent of all participants) worked an average of 513 hours in their Phase 2 jobs and 767 hours overall.

THE IMPACT EVALUATION DESIGN

The STETS research plan was designed to address five basic questions:

- Does STETS improve the labor-market performance of participants?
- Does STETS participation help individuals lead more normal life-styles?
- In what ways do the characteristics and experiences of participants or of the program influence the effectiveness of STETS?
- Does STETS affect the use of alternative programs by participants?
- Do the benefits of STETS exceed the costs?

In order to address these questions, it was necessary to obtain data that would enable us to measure what happened to STETS participants during and subsequent to their program participation and what would have happened to them over this same time period had they not enrolled in STETS. We did so by adopting an experimental design whereby eligible applicants were assigned randomly either to an experimental group (and were enrolled in the program) or to a control group (and were sent back to their referral agencies). Implementing this procedure was feasible because STETS had been introduced into the sites as a new or expanded program of moderate size (a target of 40 to 55 slots per site) relative to the size of the areas' target populations. This random assignment of applicants is an especially important feature of the evaluation design: because it yielded groups that should be similar in all respects except for their participation in STETS, it permitted relatively powerful tests of the effectiveness of STETS relative to the other options available to mentally retarded young adults in the demonstration sites.

The Sample and Data

The final research sample consisted of 437 individuals--226 experimentals and 211 control group members. The primary source of data on these individuals came from in-person interviews that were administered to sample members or their proxies immediately after random assignment and again at 6, 15, and 22 months after random assignment.[1] Because of the limited recall abilities of this target population, the surveys collected point-in-time data rather complete time-line information, such as has generally been collected for program evaluations that focus on other target groups.

These interview data were supplemented with information from a variety of other sources. Information on program-service receipt was obtained from the demonstration's Management Information System, and program cost data were obtained from the demonstration's accounting system. Other sources of data that were systematically used in the study include community service agencies, program intake records, and special work-site case studies.

Analytic Approach

The basic analytic approach used to estimate the impacts of the program entailed comparing the outcome measures for experimentals with those of controls by using regression analysis techniques. This approach enabled us to compute the overall impacts of the program and the impacts of the program on selected sample subgroups, while offering some gains in terms of the efficiency of the estimates.

A benefit-cost methodology was undertaken to assess the economic efficiency of the program. The methodology involved assigning dollar values to estimated program impacts and to program costs, and then comparing these benefit and cost estimates to yield an estimate of the program's net present value--the difference between benefits and costs, denominated in base-period values.

In applying these analytic approaches to the STETS data, our evaluation of the STETS demonstration has made two important methodological contributions to evaluating transitional-employment programs for mentally retarded persons. First, it has documented that the availability of a control group can be very important for measuring the true effectiveness of employment and training interventions for this target population. Had we been limited to using the preprogram behavior of the participant group to estimate what the behavior of participants would have been in the follow-up period had they not participated in STETS, we would have estimated substantially larger net program impacts than actually occurred. The reason is that, even in the absence of intervention, these mentally retarded young people tend both to increase their overall level of employment and to shift from noncompetitive employment settings (training jobs and workshop/activity centers) into competitive employment. Nonexperimental methodologies, which are adopted in most other evaluations, will tend to attribute some of these natural time trends to the effects of the program. The second methodological contribution of our evaluation is that mentally retarded young adults who enrolled in STETS were able to provide reasonably detailed and accurate information on their current circumstances and employment activities through in-person interviews.

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[1] Response rates to these surveys were exceptionally high. Ninety-seven percent completed the baseline, and the respective completion rates for the 6-, 15- and 22-month surveys were 95, 91, and 89 percent.

ESTIMATES OF PROGRAM IMPACTS

The analysis focused on the impacts of STETS on four major areas: (1) labor-market behavior, (2) training and schooling, (3) public transfer use, and (4) economic status, independence, and life-style.

Labor Market Behavior

The evaluation clearly indicates that a STETS-type program can be expected to improve the postprogram employment prospects of mentally retarded young adults. As shown in Table 1, employment in regular jobs was significantly greater for experimental group members than for control group members in the postprogram observation period--that is, at months 15 and 22. By month 22, experimentals were an average of 62 percent more likely than controls to be employed in a regular job (31 percent versus 19 percent), and the regular-job earnings gains were proportionately larger ($36 per week among experimentals versus $21 per week among all controls).

termined, and it is presumed that greater program efficiency is desirable. Equity questions are more difficult to answer. Benefit-cost analysis can (to some extent) determine the effect of a program on the distribution of resources, but contains no special criteria for judging whether a distributional change is desirable. Thus, addressing equity entails a more descriptive analysis, and conclusions must be based on a broader analysis of public policy and social concerns.

The basic method used to determine economic efficiency is to assign dollar values to all estimated impacts and costs. These values are then summed together to yield an estimate of the program's net present value--that is, the difference between benefits and costs, where all dollar values are adjusted to reflect their value in a specific base period. A positive net present value indicates that the resources are being used efficiently.

While the net present value criterion can easily be stated, a high degree of uncertainty surrounds its estimation: program effects are measured imperfectly, and some cannot be estimated at all; uncertainties surround the values that should be placed on the specific program effects or costs; and the appropriate techniques necessary to aggregate individual benefits and costs inherently involve numerous approximations. Consequently, it is difficult to apply the net present value criterion to judge the economic efficiency of the program.

Because of the error associated with any single estimate of net present value, much of the usefulness of benefit-cost analysis pertains to its comprehensiveness. The process of drawing together measures of the various inputs and outcomes and the general patterns that emerge from the attempts to assign relative values are often more useful than any specific estimate of net present value. For this reason, the analysis does not focus on a single net present value estimate but, instead, on a set of estimates. By examining the different assumptions, the underlying outcome estimates, and the techniques used to value outcomes, it is possible to identify those aspects of STETS and its evaluation that are most important in determining the overall findings--that is, which (if any) aspects, if changed, would change the basic nature of the findings.

The core of this approach is a benefit-cost accounting framework that imposes a logical rigor on the analysis and serves as a guide for interpreting the results. The framework specifies a consistent method for valuing the diverse sets of effects. The approach used here is based on concepts similar to those that underlie the estimation of gross national product (GNP). It focuses on the net resource gain or loss induced by STETS as it was implemented in the demonstration. Essentially, the approach entails estimating the change in resources available because of STETS, and then valuing those resources at their market cost. Thus, for example, STETS-induced reductions in the use of sheltered workshops by experimentals enables society to reallocate some of the resources that would have been used to operate those programs.[1] The market value of the saved resources is used as a measure of the value generated by the reduction in sheltered workshop use. In general, this valuation procedure is convenient to use and does not necessitate attempting to measure such difficult concepts as the willingness of society to pay for the various outcomes.

While this procedure assigns a value to program effects, that value will be viewed differently by different groups. For example, experimentals who lose SSI benefits after obtaining a job will view the loss as a cost, while taxpayers will view it as a financial gain. The accounting framework captures these differences through three analytical perspectives: society as a whole, program participants, and nonparticipants.[2] The perspective of society as a whole abstracts from all of the redistributional aspects of STETS and focuses on its efficiency, since it considers only the use of resources. Transfers of income between groups are assumed to cancel each other out in the social perspective--that is, a dollar of benefit or cost to one person is assumed to be equivalent to a dollar of benefit or cost to anyone else.[1] The perspectives of participants and nonparticipants facilitate an analysis of the distributional consequences. For each group, the question for net present value is the same as it was for society as a whole: Does net present value (from that perspective) exceed zero? Do participants gain or lose, on average, from their participation? Are their earnings gains and increased independence sufficient to outweigh the losses of transfer benefits such as SSI? How are nonparticipants affected? Does STETS require a net subsidy from nonparticipant taxpayers?[2]

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[1] Of course, one possible reallocation would be to continue to operate the sheltered workshops at the same scale and to serve clients who would not have been served in the absence of STETS.

[2] The nonparticipant group includes everyone in society who is not given the opportunity to participate in STETS. Thus, it encompasses much more than the control group, which comprises a very small part of the nonparticipant group. We prefer this term to the more common term "taxpayer group," because the participant group also pays taxes, and because not all of the effects on nonparticipants occur through the tax system.

All three perspectives mask differential effects on specific individuals or groups. The impacts of STETS are measured as averages and indicate the expected effect of STETS. Obviously, participants will differ in their response to STETS and may do better or worse than the statistical averages. In addition, individual nonparticipants will be affected differently. The employer who is able to hire a productive worker because of STETS will perceive the program differently than will an average taxpayer who helps to fund STETS. Therefore, the STETS benefit-cost estimates must be taken as indicative of the expected overall effects of STETS, viewed from a broad perspective that is appropriate for judging aggregate program performance. A more detailed analysis would be required to answer questions about whether specific types of individuals would benefit from enrolling in STETS.[1]

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[1] Of course, any resources consumed in transferring income would be counted as costs from the social perspective.

[2] One analytically useful feature of using these three perspectives is that the sum of the net present values calculated from the participant and nonparticipant perspectives equals the net present value for the social perspective. This "adding-up" property is valid because participants and nonparticipants constitute mutually exclusive groups that, when combined, include all members of society. Therefore, transfers of income between these two groups cancel each other out in the social perspective, because the benefit to one group is assumed to be equal to the cost to the other. Benefits or costs that accrue to one group and that are not offset by corresponding costs or benefits to the other (e.g., increased work output) do not cancel out when added, and they thus represent net social benefits or costs.

Because the benefits and costs of STETS occur over time, the analysis must compare streams of benefits with streams of costs. To simplify this task, we include in the accounting framework several procedures for aggregating dollars in different time periods and for producing equivalent estimates of benefits and costs at a single point in time. To do so, it is necessary to account for differences in the value of benefits and costs across time periods due to inflation and to foregone interest earnings. The inflation differences are corrected by valuing all benefits and costs in 1982 dollars. Thus, differences between benefits and costs reflect real changes in resources, not changes in the nominal value of a dollar. The differences due to foregone interest reflect the fact that a benefit that occurs in the future is worth less than the same benefit that occurs today, because today's savings could be invested and would earn interest in the future.[2] The procedure for adjusting for such differences is called "discounting," and its importance is well established among the analytic literature (see, for example, Gramlich, 1981). The only uncertainty that remains pertains to the interest rate that should be used in discounting future benefits and costs. We assume a 5 percent real annual rate (that is, a rate calculated by netting out inflation) for our benchmark, and test the importance of this assumption by calculating alternative estimates using real annual rates of 3 and 10 percent.[3]

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[1] The subgroup analyses in Chapters IV through VII provide some information on the impacts of the program on individual types of participants.

[2] For example, suppose that a $1,000 benefit occurs 10 years from now. The issue would then be, what present value invested today would yield $1,000 ten years from now? If interest rates were, for example, 5 percent, then this present value would equal $1,000 divided by $(1 + .05)^{10}$, or $614. Gramlich (1981) describes this process in more detail.

[3] The Office of Management and Budget (1972) mandates a 10 percent discount rate for evaluating government programs.

(Appendix C provides further information on the rationale and procedures for discounting.)

Table VIII.1 presents the benefit-cost accounting framework. The table lists the major impact components of STETS (regardless of whether we can value them) and suggests whether a component is, on average, a benefit, a cost, or neither from each of the three perspectives.[1] The table also indicates what data sources are used to measure and value the effects or whether a particular effect is left unmeasured. The next two sections discuss the separate cost and benefit components.

Before proceeding, however, it is important that we review how the impact estimates presented in Chapters IV to VII are used in the benefit-cost analysis. These estimates indicate the effect of STETS on experimentals at 6, 15, and 22 months after randomization. These "point-in-time" estimates are adequate measures of the impacts of STETS, but are inadequate for the benefit-cost analysis, which requires information on the impacts of the program for the entire 22 months. In order to compare benefits and costs, we need to estimate the cumulative change in earnings, program use, transfer receipt, and other activities. In the absence of continuous data on these activities, it is necessary to derive cumulative measures by interpolating between the point-in-time estimates.

Any interpolation method involves some arbitrariness. We have chosen to interpolate linearly between the point estimates. This method is straightforward, and appears reasonable in that no alternative is clearly preferable. Thus, although all program impacts used in the benefit-cost analysis are inherently more imprecise than the specific impact estimates presented earlier, we feel that estimates of cumulative effects based on linear interpolations provide an accurate indication of the true magnitude

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[1] Whether an impact component will be a net benefit or cost is sometimes problematic. Table VIII.1 reflects prior judgments about the value of components from the three perspectives. The treatment of all components in the final net present value calculations is of course determined by the estimated actual effects of STETS.

TABLE VIII.1

EXPECTED BENEFITS AND COSTS OF STETS BY ANALYTICAL PERSPECTIVE

	Analytical Perspective			
Component	Social	Participant	Nonparticipant	Data Source[a]
I. Program Costs				
Project operations	-	0	-	A
Payments to participants	0	+	-	A
Central administration	-	0	-	P
II. Output Produced by Participants				
Phase 1 and Phase 2 output	+	0	+	S
Output forgone while in STETS	-	-	0	I,P
Increased out-of-program output	+	+	0	I,P
III. Other Programs				
Reduced use of:				
Sheltered workshops	+	0	+	I,P
Work-activity centers	+	0	+	I,P
School	+	0	+	I,P
Job-training programs	+	0	+	I,P
Case-management services	+	0	+	E
Counseling services	+	0	+	E
Social/recreational services	+	0	+	E
Transportation services	+	0	+	E
IV. Residential Situation				
Reduced use of:				
Institutions	+	0	+	I,P
Group homes	+	0	+	I,P
Foster homes	+	0	+	I,P
Semi-independent residential programs	+	0	+	I,P
V. Transfer Payments and Taxes				
Reduced SSI/SSDI	0	-	+	I,P
Reduced other welfare	0	-	+	I,P
Reduced Medicaid/Medicare	0	-	+	I,P
Increased taxes	0	-	+	I,P
VI. Transfer Administration				
Reduced use of SSI/SSDI	+	0	+	I,P
Reduced use of other welfare	+	0	+	I,P
Reduced use of Medicaid/Medicare	+	0	+	I,P
VII. Intangibles				
Preferences for work	+	+	+	U
Increased self-sufficiency	+	+	+	U
Increased variation in participant income	-	-	-	U
Foregone nonmarket activity	-	-	0	U
Increased independent living	+	+	+	U

NOTE: The individual components are characterized from the three perspectives as being a net benefit (+), a net cost (-), or neither (0).

[a] The codes used for the data sources are as follows: S-special study, I-interview data, P-published data source, A-STETS accounting system data, U-item not measured, and E-item measured but excluded because the effects of STETS were trivial.

of program impacts.[1] (Details on the interpolation procedures are provided in Appendix C.)

B. STETS PROGRAM COSTS

The accounting framework disaggregates costs into three components: the operating costs of the projects, compensation paid to clients while they were in Phase 1 or Phase 2 activities, and central administrative costs. The operating and central administrative costs are paid by nonparticipants. Because these costs represent the value of the resources used to operate STETS, they also represent social costs. Participant compensation is treated as a transfer from nonparticipants to participants, because it represents a shift in resources from one group to another.[2]

1. Operating Costs

During the 27 months of operations, the five projects served 284 clients and reported operating costs of almost $2,500,000, implying average operating costs of $8,800 per client. However, for two reasons, this estimate is misleading for the benefit-cost analysis. First, it corresponds to all clients, and not to the group of 226 participants who were included in the research sample. Second, it includes costs that are attributable to the fact that STETS was a demonstration.

The first problem can be corrected in part by adjusting for differences between experimentals and other clients in their length of program participation. We estimated the cost of serving an active client for a month and then multiplied this average cost by the observed average length of time during which experimentals were active in the demonstration. This method is accurate as long as the five demonstration projects provided the same level of service per month to experimentals as they did to clients who were not in the research sample. Because there is no evidence of such differences in service provision, we feel that the method is sound.

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[1] Data on program costs and participation in STETS were obtained from demonstration accounting and client-tracking records. Because these records provided data for the full observation period, interpolation between point estimates was unnecessary for estimating the following: STETS program costs, STETS payments to participants, and the value of output produced by participants while in Phase 1 and Phase 2 jobs.

[2] The output produced in Phase 1 and Phase 2, which is related to this compensation, represents real resource gains. Section C discusses both this output and changes in output produced outside of STETS.

It is more difficult to correct for the special demonstration costs. Our general rule was to include all costs of the demonstration as it was fielded, with two exceptions. First, we subtracted an estimate of research-related costs--the costs of finding and screening applicants who were ultimately assigned to the control group, of completing and processing the research data collection forms, and of staff time spent with the researchers. Riccio and Price (1984) estimated that these costs constituted 5 percent of total project expenditures (including both operating and participant compensation expenditures). Because these research costs probably had little or no effect on the impacts of the program, we feel that they should be excluded from the benefit-cost analysis.

We made the second exception to the principle of estimating operating costs as actually incurred in the demonstration because several demonstration-specific features made the observed costs abnormally high. In particular, the limited duration of the demonstration meant that the actual costs overrepresented the higher average costs of the initial project implementation and of the demonstration phase-down period. Additional costs were also incurred because projects found it necessary to take special precautions to deal with the funding uncertainties surrounding the demonstration itself. Riccio and Price (1984) discuss these problems and suggest that the costs incurred during a five-month "steady-state" period best represent the costs of operating STETS on an ongoing basis. This period (which covers slightly different months at each site) fell in mid- to late-1982, a period when enrollments were high and operations were stable relative to earlier and later periods. We have used average costs from these periods in the benefit-cost analysis.[1]

After making these two adjustments for research costs and the effects of start-up and budgetary uncertainties, we estimate that it costs $666 per month of active participation to provide STETS services.[2] According to data collected by MDRC as part of its client-tracking system, experimentals were active for an average of 9.3 months. When discounted at a 5 percent real annual rate to the point of enrollment in STETS, the implied participation cost is $6,050 per participant.

2. Participant Compensation

Participant compensation includes the wages and fringe benefits that the five projects paid to participants in Phase 1 and Phase 2 activities. It also includes wages paid by employers directly to participants in Phase 1 and Phase 2. The projects reported all these expenditures monthly to MDRC as part of the demonstration monitoring and accounting system. During the five months of the steady-state period, participant compensation averaged $341 per month of active participation (average compensation expenditures over the entire demonstration were $30 lower than during the steady-state period). Given the estimated average length of active participation (9.3 months), this figure implies that participants received Phase 1 and Phase 2 compensation worth $3,094 per participant when discounted to the time of enrollment.

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[1] As mentioned in Chapter IV, employment impacts were greatest for persons who were served during the steady-state period, suggesting that the higher average costs during the demonstration start-up and phase-down periods occurred because the projects were establishing new procedures and dealing with inefficiencies due to small and changing scales, rather than because extra services were being provided. This finding further supports the use of steady-state costs in the benefit-cost analysis.

[2] Riccio and Price (1984) reported costs on the basis of enrollment months rather than active months. Because some clients were inactive for part of their enrollment period, the cost per enrollment month will be lower than the cost per active month. However, the average length of participation will be correspondingly higher when measured in enrollment time rather than in active time. Thus, the cost per client (the product of the cost per month and the months of participation) is independent of how time is measured. (Appendix C discusses the cost-estimating procedures in greater detail.)

3. Central Administrative Costs

Central administrative costs cover the activities necessary to administer the contracts with the five projects and to provide demonstration-wide coordination. MDRC performed this task in the demonstration, although state or federal agencies would probably assume this role in an ongoing program. For example, a state vocational rehabilitation agency could fund the programs and would assume responsibility for audit and performance monitoring. Estimating central administrative costs was difficult in STETS because of the overlap between MDRC's monitoring and research activities. Their dual role in the demonstration meant that their costs exceeded the central administrative costs that would be incurred in an on-going program. In addition, they incurred substantial start-up costs as they selected the sites and helped them operationalize the program. Consequently, the demonstration experience does not provide an adequate guide to estimating future central administrative costs. Our benchmark estimate is that central administrative costs would be approximately $20 per month if STETS were operated in a fairly decentralized manner whereby most of the monitoring would focus on audit responsibilities and fairly straightforward performance measures. Costs would be higher if the central authority provided intensive monitoring or technical assistance.

C. OUTPUT PRODUCED BY PARTICIPANTS

The analysis of STETS-induced effects on output produced by participants distinguishes between goods and services produced by participants in Phase 1 and Phase 2 and those produced by them outside of STETS. These two types of output have different distributional consequences and necessitate using different estimation techniques.

1. Value of In-Program Output

The value of output produced by participants in Phase 1 and Phase 2 is an important program benefit. This output accrues to nonparticipants (and to society) and has been very important in previous benefit-cost studies.[1] One measure of the value of in-program output is the revenue generated either by payments made by firms which used participant labor or from the sale of participant-produced goods. This valuation method provides a reasonable lower-bound estimate of the value of in-program output, because the output was actually purchased for this amount.[2] However, the STETS sites did not pursue revenue-generating strategies as their primary goal; rather, they focused on securing placement and training for the STETS participants. Thus, revenue may seriously underestimate the actual value of output. Based on estimates by Riccio and Price (1984), revenue for the five-month steady-state period was over $108,000, or $131 per active participant month. Thus, revenue offset almost 40 percent of participant compensation.

As an alternative method for estimating the value of the in-program output of participants, we conducted a series of work-activity case studies for 33 randomly selected experimentals.[3] For each person, we estimated the net value of the output they produced during a two-week reference period.[4] This estimate was based on the alternative supplier's price of the participant output--that is, on the wages and fringe benefits that would have been paid by an employer to other workers to produce the output that was produced by the participant. This estimate assumes that employers can obtain additional labor at the wages paid to their regular employees. To obtain the net value added by participants, we subtracted from the alternative supplier's price an estimate of the costs of additional employer-provided supervision and reduced output from other workers from using the participant labor.

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[1] For example, in the national Supported Work demonstration, the value of output offset approximately 65 percent of the social costs (see Kemper and Long, 1981, p. 269).

[2] Revenue is a lower-bound estimate under the assumption that profit-seeking employers would not pay more than a product (participant labor service) was worth to them. Thus, the value of output should not be less than what was actually paid. Of course, this argument is weakened if altruism prompts employers to overpay because of their desire to support the STETS program and its participants. In either event, the direct estimates of the value of output (discussed in the text) offer a more accurate estimate of the resource value of participant output.

[3] We also studied seven participants who were not included in the research sample. The results for these participants were similar to the results for the 33 experimentals (see Appendix C).

[4] The studies were completed between September and December 1982, the period that is generally considered to be part of the "steady-state" period.

These estimates of the net output are based on in-person interviews with the participants' supervisors, and rely on those supervisors' judgments about participant productivity, supervisory costs, the reactions of other staff, and the source and cost of alternative labor to perform the participants' tasks. These estimates are also subject to problems because of the difficulty in assessing whether the use of STETS-participant labor enabled the employer to increase output. There were several instances where an employer took on a STETS participant and raised the quality of the output, rather than increasing the amount or price of that output. For example, one STETS participant was hired by a day-care program operator. The operator did not change prices or increase enrollments, but, instead, apparently used the STETS participant to increase the amount of attention given to the children in the day-care program. Our net value-of-output estimates do not adjust for such quality changes. Thus, our estimates may be too low in some cases in which quality changes occurred.

Despite these limitations, the estimates of the net value of participant output represent useful indicies of the value of in-program output. They are based on careful, systematic case studies, and, where possible, incorporate actual wage, fringe-benefit, and production records in their derivation. The net-value-added estimates indicate that participants in the research sample produced output worth an average of $293 per month of active participation during Phase 1 and $503 per month during Phase 2. Given an estimated average length of active participation of 5.5 months in Phase 1 and 3.8 months in Phase 2, these figures imply a total value of output, discounted to the time of enrollment, of $3,434 per participant (an amount that would more than offset participants' in-program compensation).

2. The Value of Out-of-Program Output

STETS affected output produced outside of the program in two ways. As participants entered STETS, they gave up some alternative employment opportunities. Later, as they completed their STETS training, many participants were able to work more than would have been the case in the absence of STETS. These changes in output enter the benefit-cost analysis from the perspectives of society and participants.

Participants will perceive foregone production as a cost and increases in production as benefits. These changes also enter the social perspective to the extent that they represent a net change in total output. It is generally assumed that this is the case. However, if participants displaced workers who would have otherwise held the jobs filled by participants, then the lost output of those other workers must be subtracted from the increased output of participants in order to calculate the net change in social resources. In the extreme, STETS may have simply shuffled workers among a fixed number of jobs, with no net increase in output. The participants would have had higher incomes, but at the expense of other workers who were displaced.

At the other extreme, STETS may have enabled participants to move from a labor market in which an excess supply of labor existed to one in which an excess of demand existed. When participants leave the market in which an excess of supply exists, any jobs that they would have obtained are filled by workers who would have been unemployed otherwise. From the social perspective, this effect implies that no output is foregone by having participants enter STETS, and thus that the social cost of participation is zero. In this case, social benefits would equal the increase in participant earnings plus the increased earnings of nonparticipants who fill jobs vacated by participants. Of course, this result requires that participants not be placed back into the excess labor-supply market.

STETS jobs often seemed to be in markets in which an excess supply of labor existed. The demonstration was fielded during an economic recession, and the possibility of displacement seems relatively high, both on the jobs that they held and on those that they would have held. These indirect labor-market effects will affect the social value of the output produced in Phase 1 and Phase 2, as well as the social value of the output produced after leaving STETS.

The net change in social output in the presence of displacement can be valued in several ways. We chose to estimate net present value under the assumption that no displacement occurred, and then to assess the importance of this assumption by calculating alternative estimates under the assumption of some displacement. This approach can be thought of as indicating the potential of STETS to increase social output, provided that macroeconomic conditions are adjusted to take advantage of it. Moreover, given the absence of any empirical basis for estimating the extent of indirect labor-market effects (or even knowing the direction of their net effect), no clearly preferable alternative exists.

Under these assumptions about indirect labor-market effects, the change in out-of-STETS output was estimated as the change in total compensation received by participants (i.e., gross wages plus fringe benefits). If the markets function competitively, then the actions of employers and workers will ensure that total compensation equals the value of workers' contribution to output. In addition, regulations pertaining to wage rates in sheltered workshops require that compensation in that sector reflect productivity. To estimate total compensation, we multiplied the after-tax earnings estimates derived from interview data by a factor that reflected tax-withholding and fringe-benefit rates. This factor indicates that total compensation is 45 percent larger than after-tax earnings.[1] To ensure that the estimates correspond to out-of-program output, we used non-STETS earnings as our measure.

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[1] As described in Appendix C, gross earnings (i.e., before tax withholding) are 23 percent greater than after-tax earnings. In addition, fringe benefits for low-wage workers such as STETS participants were estimated to be 18 percent of gross earnings.

The resulting calculations indicate that at the 6-month point, when most experimentals were in STETS, the controls had $16.89 more per week in non-STETS total compensation than did experimentals. This differential was reversed at month 15, when experimentals earned $5.76 more total compensation per week than did controls. At month 22, when all experimentals had left STETS and all earnings were from non-STETS employment, the experimental-control differential in total non-STETS compensation had risen to $13.50 per week.

These estimates, along with the interpolations, are shown in Figure VIII.1 (a zero experimental-control difference was assumed for the time of enrollment).[1] The cumulative change in total compensation--which equals the value of out-of-program output--is shown as the shaded area. It indicates that, during the fifteen months after randomization, participants forewent non-STETS jobs in which they would have produced output worth $437 per participant. During the seven months between month 15 and month 22, participants produced increased non-STETS output worth $290 per participant. When discounted to the time of enrollment, these estimates imply $425 of output foregone per participant in the first fifteen months and a subsequent increase in output worth $268 per participant.

We have used these two figures to approximate both the foregone output while participants were in STETS and the increased postprogram output. Of course, since most participants spent less than fifteen months in STETS, this approximation is fairly rough. However, despite the imprecision in this disaggregation, the sum of the two estimates is an accurate estimate of the net change in non-STETS output during the 22-month observation period.

D. OTHER BENEFITS OF STETS

While the primary objective of STETS was to increase employment and earnings, the intervention also generated other important impacts. We

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[1] Details on the calculation of total compensation and the interpolation are provided in Appendix C.

FIGURE VIII.1

CUMULATIVE IMPACT ON TOTAL COMPENSATION PER PARTICIPANT FOR THE 22-MONTH OBSERVATION PERIOD

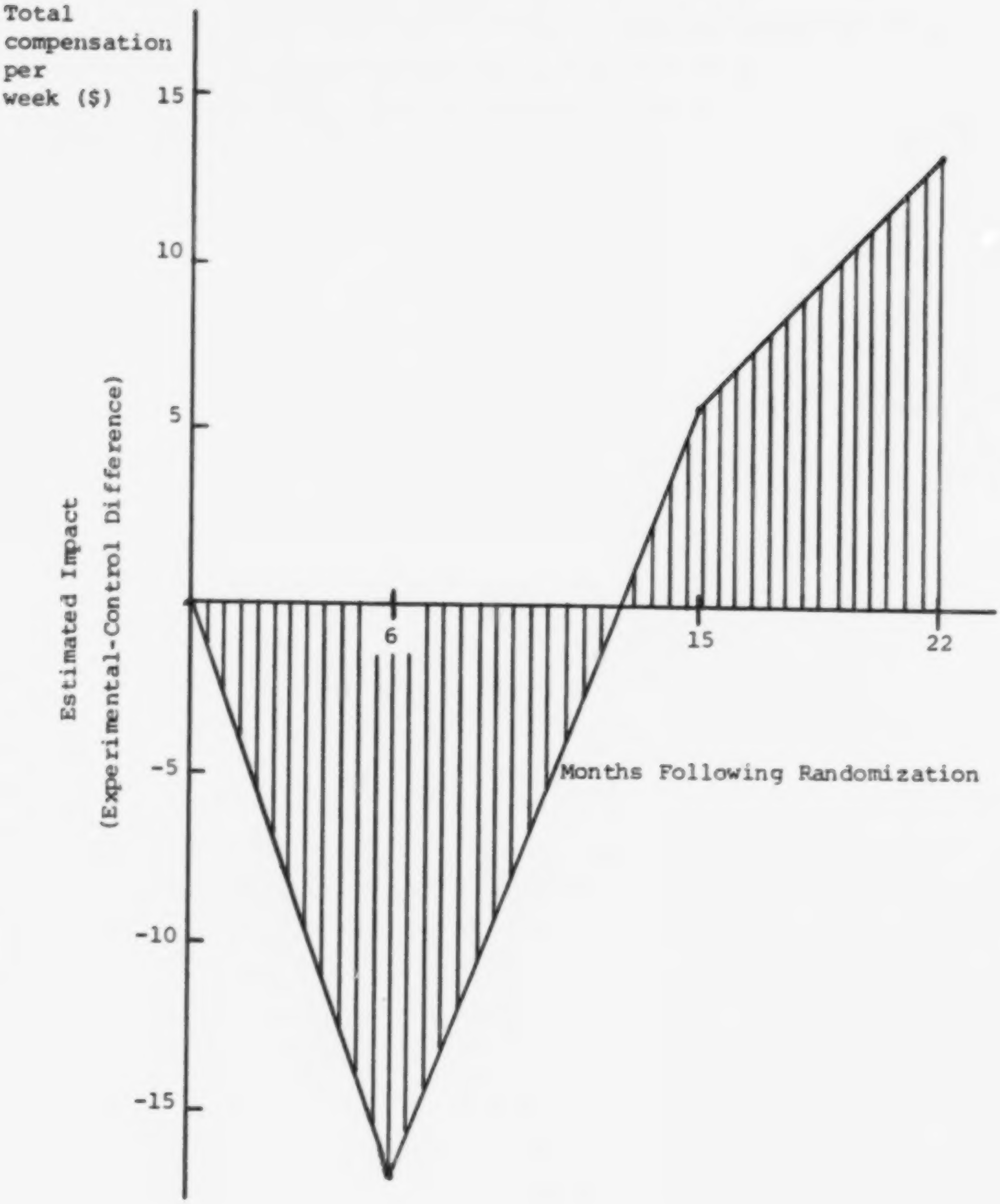

TABLE 1

ESTIMATED PROGRAM IMPACTS ON KEY OUTCOME MEASURES

Outcome Measures	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Employment									
Percent Employed in Regular Job[a]	11.8	10.7	1.1	26.2	16.8	9.4**	31.0	19.1	11.9**
Percent Employed in Any Paid Job	67.8	45.2	22.6**	44.8	43.6	1.2	44.7	43.7	1.0
Average Weekly Earnings in Regular Job	$ 11.81	$ 9.81	$ 2.00	$ 26.90	$ 16.31	$ 10.59**	$ 36.36	$ 20.55	$ 15.81**
Average Weekly Earnings in Any Paid Job	$ 52.39	$ 25.93	$ 26.46**	$ 37.91	$ 26.48	$ 11.43**	$ 40.79	$ 28.41	$ 12.38**
Training and Schooling									
Percent in Any Training	61.7	40.6	21.1**	20.6	28.4	-7.8*	16.6	29.1	-12.5**
Percent in Any Schooling	7.5	15.7	-8.2**	6.2	10.1	-3.9	8.0	11.4	-3.4
Income Sources									
Percent Receiving SSI or SSDI	26.3	31.0	-4.7	33.1	40.7	-7.6**	34.9	40.2	-5.3
Average Monthly Income from SSI or SSDI	$ 66.41	$ 74.59	$ -8.18	$ 91.35	$ 109.65	$ -18.30	$ 99.27	$ 120.03	$ -20.76
Percent Receiving Any Cash Transfers	31.7	43.1	-11.4**	44.5	51.5	-7.0*	49.6	52.0	-2.4
Average Monthly Income from Cash Transfers	$ 80.23	$ 99.98	$ -19.75	$ 114.78	$ 138.72	$ -23.94	$ 126.53	$ 136.08	$ -9.55
Average Weekly Personal Income[b]	$ 71.72	$ 50.94	$ 20.78**	$ 67.22	$ 59.67	$ 7.55	$ 71.59	$ 62.39	$ 9.20

NOTE: These results were estimated through ordinary least squares techniques. Estimated impacts or selected binary and truncated outcome measures were generated using probit and tobit analysis, respectively, with virtually identical results. (These results are presented in Appendix A to the report.)

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Personal income includes earnings, cash transfer benefits (AFDC, general assistance, Supplemental Security Income, and Social Security Disability Insurance), and other regular sources of income.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

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Furthermore, as shown in Figure 1, this postprogram increase in employment in regular jobs was roughly equal to the reduction in employment in workshop/activity centers. The STETS experience tended not to affect the average postprogram incidence of holding non-workshop-training jobs. Thus, although the overall level of employment was largely unchanged, very important compositional effects occurred. Overall, average earnings from all types of employment increased by 44 percent in month 22 ($41 per week for experimentals versus $28 per week for controls).

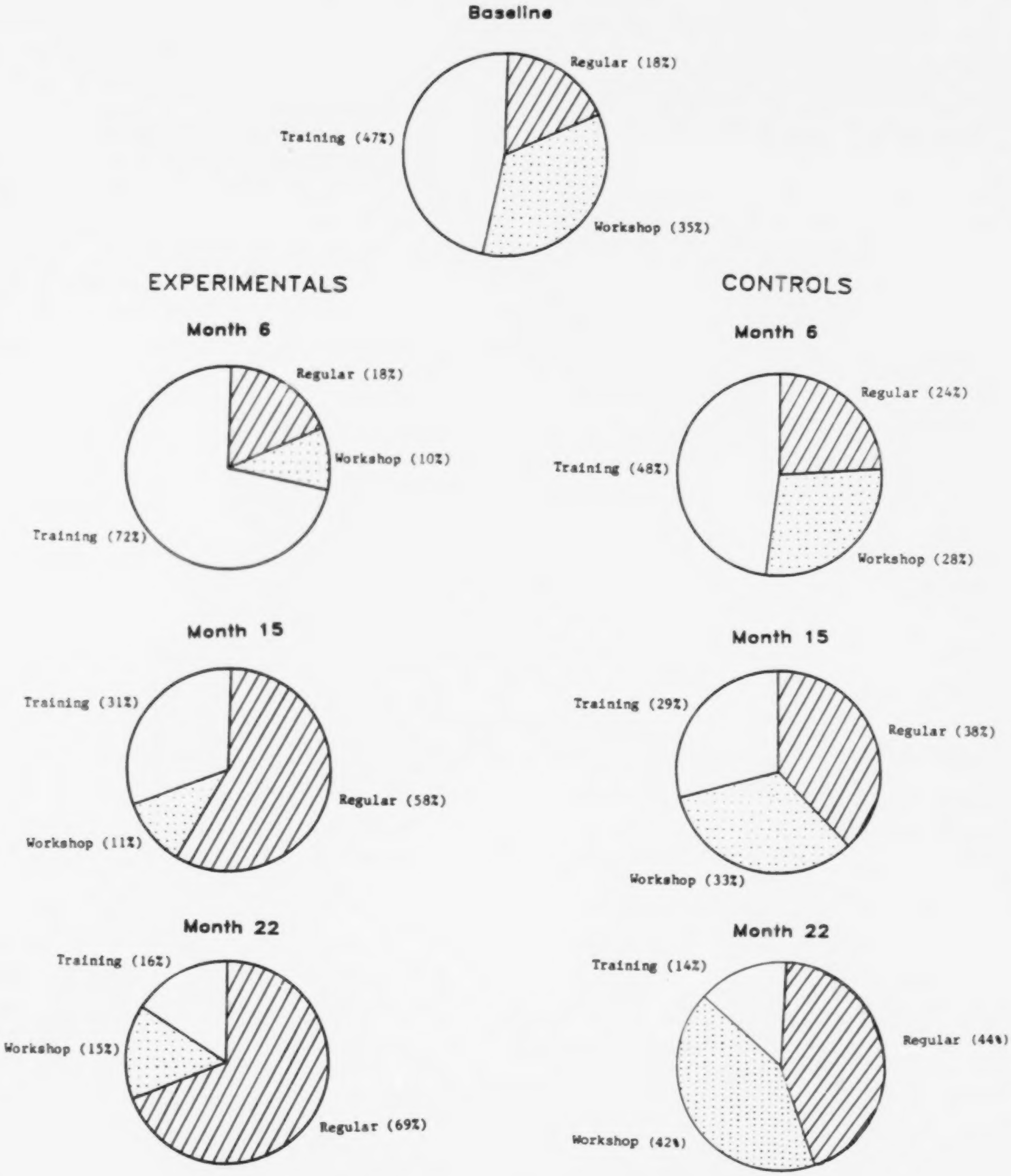
As shown in Table 2, the postprogram (month 22) results vary substantially among subgroups of the sample. The St. Paul program clearly had the largest impact on the regular job-holding of participants (41 percent of the experimentals, compared with 18 percent of the controls, held regular jobs). In terms of the differential impacts among subgroups defined by their personal characteristics, STETS seems to have been most effective for four groups: those with lower IQ scores and whose retardation has organic causes; older individuals; males; and those who were more independent, as evidenced by their living arrangements and money-management skills at the time they enrolled in STETS.

Of these results, two sets are especially thought-provoking--the results by IQ level and the results by gender. Estimated program impacts on the probability of holding a regular job were essentially zero for those participants with borderline IQ levels, 12 percentage points for those whose IQ scores indicated mild retardation, and 28 percentage points for those whose IQ scores indicated moderate retardation. The net effect of these differential program results is that STETS tended to raise the employment prospects for the mild and moderate retardation subgroups from levels that in the absence of STETS would have been well below those for the borderline retarded group to levels that were roughly similar to those of the borderline group.

The impact estimates for males and females show a clear pattern--STETS had substantial impacts on the employment and earnings of males but no impacts on the employment and earnings of females. For example, in Month 22, both male and female participants would have earned an average of

FIGURE I

TRENDS IN TYPES OF JOBS HELD BY EMPLOYED EXPERIMENTALS AND CONTROLS



NOTE: This figure is based on regression-adjusted data.

TABLE 2

ESTIMATED PROGRAM IMPACTS ON KEY OUTCOMES TWENTY-TWO MONTHS AFTER ENROLLMENT FOR KEY SUBGROUPS OF STETS PARTICIPANTS

Subgroups Defined by Characteristics at Baseline	Percent in Regular Job[a]			Average Monthly Income from SSI/SSDI			Total Weekly Personal Income		
	Experimental Group Mean	Control Group Mean	Estimated Impacts	Experimental Group Mean	Control Group Mean	Estimated Impacts	Experimental Group Mean	Control Group Mean	Estimated Impacts
Total	31.0	19.1	11.9**	$ 99.27	$ 120.03	$ -20.76	$ 71.59	$ 62.39	$ 9.20
Site									
Cincinnati	17.9	1.6	16.3	67.34	105.07	-37.73	49.84	33.77	16.07
Los Angeles	24.7	9.3	15.4	180.58	207.39	-26.81	75.76	59.66	16.10
New York	43.4	32.2	11.2	85.42	126.11	-40.69	88.96	84.59	4.37
St. Paul	41.1	17.9	23.2*	49.65	30.69	18.96	76.45	59.47	16.98
Tucson	29.6	30.8	-1.2	93.74	96.90	-3.16	65.85	68.46	-2.61
IQ Level									
Borderline	34.1	28.7	5.4	89.70	83.38	6.32	75.59	58.00	17.59
Mild	28.1	16.0	12.1**	98.02	143.75	-45.73**	66.21	67.31	-1.10
Moderate	38.2	10.7	27.5**	131.85	85.16	46.69	89.95	46.55	43.40**
Age									
Younger than 22	30.2	22.2	8.0	85.92	112.09	-26.17	68.84	60.98	7.86
22 or older	32.5	12.2	20.3**	129.21	137.86	-8.65	77.81	65.57	12.24
Gender									
Male	35.5	18.2	17.3**	91.46	133.93	-42.47**	80.41	63.46	16.95**
Female	25.1	20.3	4.8	109.26	102.24	7.02	60.23	61.00	-0.77
Race/Ethnicity									
Black	28.3	18.9	9.4	67.30	92.50	-25.20	63.34	55.79	7.55
Hispanic	48.4	25.8	22.6*	94.36	94.58	-0.22	87.20	64.85	22.35
White and other	29.7	19.2	10.5*	118.04	141.68	-23.64	72.06	65.35	6.71
Living Arrangement									
Living with parents	33.5	18.7	14.8**	102.13	111.08	-8.95	73.38	59.63	13.75**
Living in supervised setting	12.2	24.6	-12.4	78.11	102.77	-24.66	55.94	69.55	-13.61
Living independently	29.3	16.8	12.5	96.81	229.20	-132.39**	72.50	81.26	-8.76
Financial Management Skills									
Independent	41.9	23.9	18.0**	92.74	122.46	-29.72	91.76	68.78	22.98*
Not independent	26.9	17.3	9.6*	101.73	119.12	-17.39	63.90	59.95	3.95
Receipt of Transfers									
SSI/SSDI	27.7	14.5	13.2*	203.06	180.55	22.51	98.20	72.80	25.40**
Other transfers only	42.1	16.3	25.8**	53.91	114.13	-60.22**	69.86	60.15	9.71
No transfers	23.4	26.4	-3.0	36.12	63.66	-27.54	46.39	54.04	-7.65
Cause of Retardation									
Organic	33.6	9.9	23.7**	92.91	186.57	-93.66**	78.37	67.94	10.43
Nonorganic	30.4	21.0	9.4**	100.56	106.52	-5.96	70.21	61.26	8.95
Work Experience in Two Years Prior to Enrollment									
Regular job lasting ≥ 3 months	52.3	20.9	31.4**	96.19	141.25	-45.06	84.84	84.45	0.39
Other job lasting ≥ 3 months	27.9	31.2	-3.3	106.51	87.67	18.84	71.99	62.12	9.87
Other	26.9	10.5	16.4**	95.34	135.51	-40.17**	67.50	56.22	11.28
School Status at Referral									
Enrolled	27.0	14.0	13.0*	116.55	132.02	-15.47	66.86	59.19	7.67
Not enrolled	31.9	21.4	10.5*	91.36	114.55	-23.19	73.76	63.86	9.90
Total in Sample			395			399			392

NOTE: These results were estimated through ordinary least squares techniques. The control variables included in the models which underlie the overall net impact estimates are defined in Appendix A to the report.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

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about $18 to $20 per week on regular jobs ($107 per week among those employed) in the absence of STETS, as evidenced by the earnings of the control group members. However, participating in STETS raised the average regular-job earnings of males by 144 percent, to $48 per week ($134 per week among those employed). The average regular job earnings of females in the experimental group were comparable to those of their control-group counterparts in month 22 (about $21 per week, on average). These differential results for males and females seem to be related to the observed greater difficulty of the programs in placing females in Phase 2 program jobs, especially during the non-steady-state periods of program operations.

Training and Schooling

The employment impacts that were estimated for STETS were hypothesized to lead to reductions in the use of other training programs and schooling. Such an outcome is important because many forms of training and schooling for mentally retarded young adults are expensive, long-term programs that do not have a strong record of placing individuals into competitive employment.

A negative program impact on schooling was evident at month 6 as the activities of experimentals were absorbed by STETS; a smaller negative impact persisted in the postprogram period. However, the pattern of impacts on training is more complicated. Although controls used training programs to a great extent during the early months after their application to STETS, experimentals used such programs to an ever greater extent during their in-program period, since STETS, itself, was a training program. However, as expected, STETS did substantially reduce the use of training (primarily workshop-related training) in the postprogram period by 43 percent, an amount roughly equal to the postprogram increase in regular employment (see Table 1). Thus, the STETS-induced increase in employment did carry with it overall resource-cost savings from the reduced use of training programs and schooling.

Public Transfer Use

The hypothesized employment impacts were expected to lead to reductions in the use of public transfers. Although some evidence suggests that STETS did reduce dependence on cash transfers, the results are smaller than were initially expected. The impacts shown in Table 1 are reasonably large for the in-program period, with a 26 percent decrease in the receipt of any cash transfers (32 percent for experimentals versus 43 percent for controls). However, this impact faded over time, until no statistically significant impact was evident by month 22 (although the point estimate is a reduction of 3 percentage points in transfer use and an average of nearly $10 per month in benefits).

It is noteworthy that relatively larger persistent reductions were estimated for the portion of cash transfers accounted for by Supplemental Security Income and Social Security Disability Insurance. (The percentage decrease in the receipt of these transfers hovered around 15 points.) In month 22, the $21 per-month average reduction in SSI/SSDI benefits offset about 40 percent of the average $54 per-month earnings gains; at the same time, benefits from other cash transfers (primarily welfare benefits) increased by 50 percent, from $19 to $29 per month, due at least in part to the increased receipt of unemployment insurance benefits.

The subgroups that were responsible for the small overall reductions in cash transfers are generally those that experienced reductions in SSI/SSDI income. As shown in Table 2, these subgroups are often, but not always, the same subgroups that experienced the greatest gains in regular employment from participating in STETS.

Economic Status, Independence, and Life-Style

Employment impacts were also expected to influence socio-economic status, independence, and life-style. STETS did raise the total personal income of participants, but by less than would be indicated by the earnings gains, since the earnings gains coincided with reductions in cash transfer benefits, as was noted above. In the postprogram period, STETS increased total personal income by between $8 and $9 (see Table 1). Perhaps because of the modest level of this increase in personal income (generally among the same groups who were moving into regular employment, as shown in Table 2), we failed to detect any strong patterns of program impacts in the postprogram period on measures of independence and life-style. Admittedly, measures of independence and life-style are less well defined than are measures of the other outcomes under evaluation. However, it is likely that the income gains for most experimentals were too small to effect measurable changes in other aspects of independence or life-style; and, for those with more substantial income gains, the observation period may simply have been too short to expect that the gains would translate into long-term adjustments in these areas.

RESULTS OF THE BENEFIT-COST ANALYSIS

The benefit-cost analysis suggests that programs such as STETS can be a worthwhile social investment. On average, it cost $9,400 per participant to provide the STETS intervention, of which $3,100 represents wages paid to the participants in Phase 1 and Phase 2 jobs.[1] During the 22-month observation period, this investment yielded increases in output and reductions in the use of other programs by participants that offset about 85 percent of this initial investment. Consequently, we observed a measured net social cost of about $1,000 per participant for the period covered by our data. However, trends observed for the impacts on earnings and the use of sheltered workshops suggest that benefits will persist and are likely to outweigh costs in the long-run. If the earnings gains and benefits from the use of reduced alternative programs continued undiminished for as little as seven months beyond the 22-month point, social benefits would exceed social costs. This finding is consistent with other successful training programs that incurred substantial up-front costs for training in order to generate long-run benefits.

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[1] This average cost per participant compares favorably with the average costs of other similar programs targeted toward mentally retarded populations and other disadvantaged groups (see Thornton and Maynard, 1985, for such a comparison).

In addition, the STETS investment created intangible benefits by increasing the overall employment and social opportunities available to the participants. While the evidence is limited, the fact that many participants tended to remain voluntarily on their jobs suggests that STETS enhanced the quality of their lives. Furthermore, the increased income, regular job-holding, and social interaction provided by STETS are also expected to represent benefits.

Because of their increased earnings both during and after STETS, participants benefitted substantially from the intervention. On average, they received a net benefit during the 22-month observation period of $2,100 per participant, and this amount is expected to grow as the participants continue to work.

From the perspective of nonparticipant taxpayers, STETS required a substantial investment of $3,100 per participant during the observation period. This group paid not only the program operating costs, but also the in-program participant-compensation expenditures. Approximately two-thirds of these costs were offset by output produced by participants while in STETS, by savings from the reduced use of sheltered workshops and other programs, and by savings in transfer payments (primarily SSI/SSDI). If these impacts persist at their 22-month levels, then the nonparticipant costs will be entirely recouped within four and a half years after randomization. Even if measured benefits to nonparticipant taxpayers fall short of their costs, STETS may still be an attractive investment because it effectively achieves a widely stated goal of public policy--to increase the employment opportunities and performance of mentally retarded young adults.

POLICY CONCLUSIONS

The results of this evaluation demonstrate that many mentally retarded young adults can indeed perform adequately in competitive employment situations, sometimes with minimal or no ongoing support services. More importantly, the results of this study indicate that transitional-employment services such as were provided by STETS can be very instrumental in helping mentally retarded young adults achieve their employment potential. The main policy conclusions that evolve from this evaluation of the

STETS demonstration pertain to (1) the potential of STETS to mitigate the employment and independence problems of mentally retarded young adults; (2) key features of the program design and targeting strategies; and (3) the benefits and costs of the program.

Program Potential

STETS-type programs can be expected to have no impact on the overall employment level of mentally retarded young adults who are recruited through methods similar to those used in this demonstration. However, they will tend to move workers out of workshops and into competitive employment. While the net gain in unsubsidized employment attributable to STETS is about 12 percentage points, this overall result represents aggregate impacts and masks the substantially larger effects for selected subgroups of the target population. Even at this apparently modest overall figure, the estimated impacts represent a movement into competitive employment of 60 percent of those who would otherwise be in workshops or activity centers. Furthermore, if one looks at the effects during the steady-state period of operations, when the programs successfully served both males and females, the overall employment impacts are about 25 to 30 percent larger.

These programs can be expected to reduce public outlays for SSI/SSDI and education and training services substantially (especially sheltered workshops). However, partially offsetting increases in outlays for other cash transfer programs can also be expected.

No clear evidence exists to indicate that aspects of life-style or social well-being will improve. However, since participation in this program was voluntary, one can assume that most, if not all, participants placed some nonpecuniary value on obtaining regular employment.

Program Design and Targeting

Several noteworthy results from this evaluation pertain to issues of program design and targeting. First, evidence clearly suggests that ongoing programs might well achieve substantially greater success than was observed for the full STETS demonstration, since we found much larger estimated employment impacts when we compared the employment behavior of experimentals and controls who participated in the program during the "steady-state" period--an approximately five-month period when project operations were relatively stable. Second, Phase 2 activities (the supported employment and training experience within a competitive employment setting) were a key part of the program treatment. Third, in selecting participants, programs should not "cream" from among those who have no work-disabling secondary handicaps. As has been found in evaluations of employment and training programs targeted toward other segments of the population, STETS was often very successful with those whose prospects for entering competitive employment in the absence of the program were the lowest. One notable characteristic that identified such groups was an IQ score that indicated mild to moderate retardation.

One caveat with respect to the targeting issue is the strikingly more favorable program impacts for males than for females, even though both groups are expected to have quite similar success rates in entering competitive employment in the absence of STETS. In view of the estimated importance of Phase 2 activities, the fact that a disproportionate share of female participants relative to males never entered Phase 2 employment may explain, in part, the lack of significant employment impacts for females. Obviously, further investigation of the nature of these greater difficulties in serving females is warranted. However, the results of this study suggest that a major factor that affects the ability of program operators to meet the greater challenge presented by female participants is simply whether their attention is diverted by major program operational changes, such as start-up or phase-down activities; in the steady-state, their success with females was only slightly lower than their success with males.

Benefits and Costs

In the long-run, STETS-type programs are probably a socially justifiable investment, in as much as the total benefits (the net increase in the value of the goods and services produced) will outweigh the costs of the program. However, as is generally the case, taxpayers must be willing to make a substantial up-front investment that may not be repaid to society or to the taxpayers for several years. Using the full STETS sample and assuming some modest decay (5 percent per year) in the postprogram results, this pay-off period to society is estimated to be about two and a half years, and the pay-off period to nonparticipants (tax payers) would be about four and a half years. However, if the effectiveness of ongoing programs were more similar to the effectiveness of STETS during its "steady-state" operations, this pay-back period could be substantially shorter.

Generalizability of the Findings

In view of the evaluation design, the results of this study are quite robust and provide reliable estimates of what one would expect upon replicating the STETS demonstration. However, the study findings are limited in two respects. First, they are based on only five, judgmentally selected urban sites, where programs were specially designed and implemented for this demonstration. We cannot be certain whether other program operators in other sites and who operate on-going programs under different social, political, and economic conditions would have similar experiences. (For example, economic conditions were generally poor during the period of this demonstration.) It is also problematic whether similar programs could be efficiently and effectively operated in rural areas or even in more dispersed labor markets. Second, the participants represent only a small portion of the areas' populations of mentally retarded young adults, with most of the screening and selection having occurred at the social service agencies that made referrals to STETS. The evaluation design did not enable us to estimate or, more importantly, to characterize the selection processes and participation decisions. Thus, we are unable to predict the total portion of the target population that could potentially be moved into competitive employment through the provision of STETS-type services.

Despite these limitations with the study, the overall conclusion that transitional employment is an effective program for increasing competitive job-holding among mentally retarded young adults is quite clear. In fact, this program appears to be among the more effective employment and training initiatives that have been field-tested. For example, these results compare favorably with the often-cited examples of successful employment and training programs (Supported Work for the long-term welfare-dependent population and Job Corps),[1] and they are much more favorable than the results for youth-orientated initiatives other than Job Corps.[2]

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[1] See Hollister et al. (1984), and Mallar et al. (1982).

[2] See, for example, Maynard (1984), Bassi and Simms (1983), and Dickinson et al. (1984).

IMPACTS OF TRANSITIONAL EMPLOYMENT FOR MENTALLY RETARDED YOUNG ADULTS: RESULTS OF THE STETS DEMONSTRATION

I. TRANSITIONAL EMPLOYMENT FOR MENTALLY RETARDED YOUNG ADULTS

The growing interest in the concept of transitional employment reflects an increased recognition of the desirability and potential effectiveness of transitional-employment services in mitigating the employment problems of mentally retarded individuals. Twelve years ago, systematic efforts to help mentally retarded individuals obtain and maintain jobs in the competitive labor market were very limited. Since then, growing public support for hiring mentally retarded (and other handicapped) persons and advances in training and production technologies have made it easier for mentally retarded individuals to obtain and hold competitive jobs. Moreover, the outcomes of dozens of small prototype transitional-employment programs have helped change perceptions about the capacity of mentally retarded individuals to engage in competitive employment (see, for example, Bellamy, Horner, and Inman, 1979; Rusch and Mithaug, 1980; Wehman and McLaughlin, 1980; and Wehman, 1981).

Of course, as the number of programs that offer the transitional-employment approach have grown, so has the demand for program accountability. Legislators, administrators, mentally retarded persons, and their advocates all want to know the effectiveness of this type of intervention. Although competitive employment is now a widely accepted goal and although the feasibility of the approach has been demonstrated, concern remains about whether transitional-employment programs for mentally retarded persons should be expanded further. Questions remain about the magnitude of program effects and whether those effects are sufficiently large to justify program costs. Moreover, whether these programs can be replicated in new settings and whether they can operate at a size relevant to large-scale implementation are still major concerns.

The purpose of the Structured Training and Employment Transitional Services (STETS) demonstration, funded at the national level by the U.S. Department of Labor, was to address these concerns by providing a rigorous test of transitional employment for mentally retarded young adults. This multi-year project, which was overseen by the Manpower Demonstration

Research Corporation (MDRC), operated transitional-employment programs in five cities nationwide and included a rigorous evaluation that followed a sample of almost 450 experimental and control-group members for twenty-two months after program application.

The large-scale, rigorous evaluation design and the relatively long follow-up period distinguish the STETS demonstration as the most comprehensive test of transitional employment yet undertaken. It provides powerful information on the three principal policy issues surrounding transitional employment:

1. **Implementation.** Can the STETS model be implemented on a policy-relevant scale? What are the advantages and disadvantages associated with alternative variations of this basic model?

2. **Impacts.** Does STETS improve the labor-market performance of its participants (compared with what it would have been in the absence of STETS)? Does STETS affect its participants' use of alternative training, education, social service, and income-support programs? Does STETS participation help persons lead life-styles that are more typical of the population norm? Is the program more effective for participants who exhibit particular characteristics or who have had certain experiences?

3. **Benefits and Costs.** What does it cost to operate a transitional-employment program on a policy-relevant scale? Are the outcomes sufficiently large to justify these costs?

The implementation issue was analyzed by Bangser and Price (1982), who examined how the STETS projects were established, and by Riccio and Price (1984), who examined the demonstration program operations and developed suggestions for replicating the model. The impact and benefit-cost analyses are presented in this report.

As shown in this report, STETS did succeed in improving the postprogram employment prospects of mentally retarded young adults. At a point nearly two years after they entered the program and, on average, a year after they left it, former participants were 62 percent more likely to hold a regular job than had they not entered the program, and the earnings gains associated with those jobs were proportionately greater. This increase in regular job-holding was largely concomitant with a reduction in workshop-related activities. Primarily because of these results, we estimated substantial social benefits from STETS. These benefits were large enough that their value would exceed program costs from the perspective of society if the observed trends were to continue for as little as seven months beyond the observation period. For participants, benefits clearly exceeded costs in the observation period. For nonparticipants, who paid the taxes to fund STETS, the point at which benefits might exceed costs is difficult to project, but the continuing savings to nonparticipants from the reduced use of sheltered workshops suggests that a few more years of savings at the observed rate would be required. Our findings in other areas (primarily transfer program use, independence, and life-style) are less conclusive. The reasons are unclear, but it may well be that behavioral changes in these areas require a longer observation period in which employment and earnings gains are consolidated and are perceived as long-term gains.

Chapter II presents the key elements of the demonstration design. In particular, it describes the basic STETS program model, the research focus, and data collection design. Chapter III explains how the design was implemented and describes the demonstration sample members, the success of the research data collection effort, and the analytical methods used in the evaluation. Chapters IV through VII examine the specific impacts of the STETS intervention. Each of these impact-analysis chapters begins by describing the expected activities of experimentals had they not entered STETS, and then analyzes the impact of STETS both overall and among specific subgroups. Chapter IV discusses the impacts of the program on employment, earnings, and other labor-market behavior; Chapter V discusses the results on training and schooling; Chapter VI presents the findings on public assistance dependence; and Chapter VII addresses the estimated impacts on economic status, independence, and life-style. Chapter VIII assesses the benefits and costs of STETS. Finally, Chapter IX summarizes the various results of the impact and benefit-cost analyses and discusses their implications in terms of public policies targeted toward this population of mentally retarded young adults. Additional details on specific impact estimates, survey and data collection issues, and benefit-cost evaluation procedures are provided in Appendices A, B, and C, respectively.

The remainder of this introductory chapter reviews the policy context of the demonstration. It begins by reviewing the evolution of public policy toward handicapped persons, particularly mentally retarded individuals. It then discusses the role of transitional services in helping disabled persons become economically and socially self-sufficient.

A. PUBLIC POLICY, EMPLOYMENT, AND MENTALLY RETARDED PERSONS

Until the late 1960s and early 1970s, mentally retarded persons had little opportunity to enter the mainstream of society. The more severely disabled persons were particularly likely to be segregated from their nondisabled peers. Former HEW Secretary Joseph Califano characterized the situation by noting[1] --

> For decades, handicapped Americans have been an oppressed and, all too often, a hidden minority, subjected to unconscionable discrimination, beset by demoralizing indignities, detoured out of the mainstream of American life and unable to secure their rightful role as full and independent citizens.

In this environment, little attention was paid to employment opportunities for mentally retarded persons. Education programs for this population were oriented little or not at all toward vocational training or employment (Moss, 1980). Furthermore, although sheltered workshops dated from at least the mid-1840s, community workshops specifically for mentally retarded adults were relatively rare until the 1950s. And those that were implemented were operated as if competitive employment were impossible for virtually all mentally retarded persons.

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[1] Remarks made on April 29, 1977 (see Mayer, 1982).

This situation changed dramatically in the 1970s. In response to growing advocacy by and for disabled persons, several landmark pieces of legislation were passed that reflected a major change in policy and the recognition of the rights and abilities of these members of society. The Rehabilitation Act of 1973 (P.L. 93-112) and its amendments prohibited discrimination against the disabled in programs and activities sponsored under federal financial assistance.[1] The Vocational Education Act (P.L. 94-482) and provisions of the Comprehensive Employment and Training Act (P.L. 94-524) encouraged the participation of handicapped persons in vocational training and employment programs, while the Revenue Act of 1978 provided tax credits for hiring handicapped workers. In addition, a number of court cases and growing pressure from advocates (and state budgets) led to the deinstitutionalization of many mentally retarded adults (see Scheerenberger, 1981).

The centerpiece of this body of legislation is the Education for All Handicapped Children Act of 1975 (P.L. 94-142). This law, sometimes referred to as the "Bill of Rights for the Handicapped," led to the rapid expansion of special-education facilities, staff, and curricula. In particular, it requires that all handicapped children be provided with a free and appropriate education, as well as with the necessary related services (such as transportation, speech pathology, and psychological assistance). Thus, all handicapped persons from ages 3 through 21 are to be provided with an appropriate educational program, regardless of the nature or severity of their handicap. The legislation also requires that, to the extent appropriate, the education be provided in the least restrictive environment--that is, handicapped students should receive education in the same environment as nonhandicapped children, unless they are unable to benefit from the education in such a setting.

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[1] In addition to prohibiting discrimination (Section 504), this act also mandated that architectural barriers be eliminated (Section 502), and that most federal contractors take affirmative action to employ the handicapped. However, although the Act was passed in 1973, the implementing regulations (45 CFR 84.1 et seq.) were not published until 1977.

Through these laws, society has come to recognize the abilities and contributions of mentally retarded persons. However, the actual implementation of these laws has also left us with a renewed realization of the severity of the problems facing mentally retarded persons and the difficulties inherent in developing effective public policy for dealing with them. Even when President Ford signed the Education for All Handicapped Children Act, he remarked, "Unfortunately, this bill promises more than the federal government can deliver." Since then, federal appropriations have fallen well below authorized levels, and state and local governments have been struggling to provide the required special education services. Furthermore, particularly important to this study, funding adult services has become even more problematic. State departments of developmental disabilities and vocational rehabilitation face severe funding constraints and generally do not have the resources available to provide the long-range services and individualized programming required by mentally retarded individuals to become and remain self-sufficient. In terms of employment, sheltered workshops remain the major alternative available, and these facilities are typically crowded and often have long waiting lists. In addition, they have traditionally been characterized by low wages and slow movement toward nonsheltered employment (U.S. Department of Labor, 1979).

The goal of public policy during the next ten years will be to maximize the efficiency of this service system so as to reduce the discrepancy between the promises of earlier legislation and the reality of current adult services. The constituency for these adult services is already large and continues to grow. In 1976, over 88,000 mentally retarded persons participated in sheltered workshops (U.S. Department of Labor, 1979). Many of these workshop participants want and do have the capacity to work in regular jobs. In addition, the schools will graduate many more mentally retarded young adults who can be expected to seek jobs as they leave special education programs that have stressed employment.[1]

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[1] In school year 1982-1983, 24,000 mentally retarded students between the ages of 18 and 21 were being served under P.L. 94-142 (U.S. Department of Education, 1984a).

The goal is to establish efficient programs and policies that enable mentally retarded persons (as well as persons with other disabilities) to participate in the mainstream of society to the fullest extent possible.

B. TRANSITIONAL EMPLOYMENT AND RELATED SERVICES

Transitional-employment programs represent one promising vehicle for enabling mentally retarded persons to become more self-sufficient. As noted earlier, these programs have developed rapidly during the last ten years, from a few small prototype efforts to several large statewide programs. Before we describe the STETS model of transitional employment and our evaluation of its effects, it is useful first to define the concept of transitional employment, and then to review previous experience in implementing the concept and the research on its effectiveness.

1. What is Transitional Employment?

The term "transitional employment" has been applied to programs that differ widely in their structure and in the specific manner in which they provide training. Nevertheless, these programs share several common features. A recent planning meeting at the U.S. Department of Education (1984b) defined transitional-employment programs (TEPs) as follows--

> TEPs are short-term interventions that lead to the employment of service consumers. These programs aim to achieve employment in competitive jobs at or above the minimum wage, subsidized only by temporary job credits and similar incentives to employers to hire people with disabilities. TEPs emphasize employment in regular work places where non-disabled people work. They normally involve a goal of full time work (35+ hours per week) at the close of training.[1]

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[1] Some interest has also been expressed recently in "supported employment." This model, which emphasizes permanent or long-term subsidized employment, is intended to serve severely handicapped individuals who may never be able to hold an unsubsidized competitive job. However, it does emphasize work in settings (or enclaves) in which the individuals perform productive work and interact with nonhandicapped persons. Adding to the possible confusion is the term "supported work," which has been used to describe transitional-employment programs for both mentally retarded persons (see, for example, Wehman and Kregel, 1984) and other disadvantaged populations (see, Hollister, Kemper, and Maynard, 1984).

Three aspects of this definition should be emphasized. First, the essence of transitional employment is that it is a short-term intervention. Its purpose is to provide a bridge to employment, not permanent support for employment. The objective is to provide intensive training and job placement in an effort to move persons into competitive employment, and to enable them to remain employed and become self-sufficient (although it is recognized that some persons will lose their jobs and may again need program services).

In order to adhere to the short-term orientation and to promote client independence, transitional-employment programs follow a plan whereby each client's training is gradually phased out. In phasing out the formal training activities, the programs continue to provide a variety of support services, including counseling, crisis intervention, and, if necessary, additional training. These follow-up services may, for example, help clients cope with a new manager, deal with disillusionment or confusion about the job in particular or working in general, handle relationships with co-workers, and adapt to new job tasks. Moreover, the services may also help an employer train supervisors to work with handicapped individuals or to restructure job tasks to fit the abilities of disabled workers.

The length of time during which clients receive these follow-up services varies. Some programs (such as those operated by Moss, 1980, and Hill and Wehman, 1983) do not formally terminate services to clients. The programs continue to provide follow-up services long after a person is trained and placed in a competitive job. These services are often minimal (Moss estimated that long-term follow-up services cost $10 per client month), but they provide support that is deemed necessary to ensure long-term employment. Other programs adhere more strictly to the short-term objective of transitional employment, but they usually provide some placement or counseling services to former clients.

Second, the definition of transitional employment embodies an emphasis on procuring work in the unsubsidized, competitive job market. This focus reflects the goal of the programs to move persons into the mainstream of society. It is expected that these types of jobs will provide the interactions with nonhandicapped peers and the earnings necessary to integrate persons into normal society. In comparison, many sheltered workshops generate employment opportunities, but offer little interaction with nonhandicapped persons and, in general, very low wages.[1]

Third, most transitional employment for mentally retarded persons is oriented toward providing some training on the job that the person is expected to keep. By providing training in the work setting, programs try to minimize problems that may be experienced by trainees in generalizing newly acquired skills to different settings. This focus also enables transitional-employment programs to work with job supervisors and co-workers in helping to establish the trainee in the workplace. Furthermore, training on the job means that the skills and behaviors that are taught can be tailored precisely to the trainee's job tasks and work environment.

As part of the training, transitional-employment programs focus not only on job-specific work skills but also on social skills. Research has shown that social skills are as important as work abilities in determining job retention (see Gold, 1973; Wehman, 1975; and Rusch, 1979). Thus, programs provide assistance and guidance in self-care and appearance, relations with co-workers, transportation, functional reading, and communication. Programs also provide social support services that range from assistance in managing finances to assistance in obtaining housing and interacting with public assistance and other social-service programs.

2. Experience with Transitional-Employment Programs

The first efforts to implement a transitional-employment program, as defined here, were university-based programs, some of which include the university food-service program operated by James Moss at the University of Washington; the University of Illinois Food Service Training Programs directed by Frank Rusch; Project Employability, directed by Paul Wehman in Richmond, Virginia; project EARN, directed by Paul Bates at Southern Illinois University; and the Mid-Nebraska Mentally Retarded Services Program directed by Robert Schalock and Harry Drake in Hastings, Nebraska.[1] These and other, similar programs concentrated on developing appropriate training methods for mentally retarded individuals and on establishing the feasibility of transitional-employment programs. These efforts tended to be small, often training fewer than ten individuals at a time, and generally required intensive involvement by highly trained staff.

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[1] In 1976 (the most recent year for which comprehensive data are available), the average hourly wage rate in certified workshops was $0.81; the average for persons in work activity centers was only $0.43 (U.S. Department of Labor, 1979). At that time, the federal minimum wage was $2.30 per hour.

In addition to these university-based programs, a number of transitional-employment training programs have been operated by foundations and private organizations, including those operated by the Association for Habilitation and Employment of the Developmentally Disabled (AHEDD) in Harrisburg, Pennsylvania, by Job Path in New York City, by the Menninger Foundation in Kansas, and by Transitional Employment Enterprises (TEE) in Boston, Massachusetts. These efforts were generally larger than the university-based programs, tended to serve less severely disabled individuals, and were more transitional in nature. These programs generally developed their own training protocols, often expanding upon and modifying techniques developed in employment and training programs for more general populations.

In addition to these local programs, the Association for Retarded Citizens of the United States (ARC-US) has operated a national on-the-job training (OJT) program funded by the Department of Labor (see Stumbaugh, 1982). This program provides a very modest intervention whose purpose is to increase incentives to employers to hire mentally retarded workers. The program offers eight weeks of wage subsidies in order to reimburse employers partially for the costs of on-the-job training provided to newly hired mentally retarded workers. In 1984, this program served 2,300 clients and had a budget of over $1.16-million. It has served over 30,000 workers since its inception in 1967. This program may be limited by the short duration of the intervention and by a lack of trained staff to help the employers provide on-the-job training. However, the program's wage subsidies have been used widely by more intensive transitional-employment programs to supplement their efforts.

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[1] Moss (1980) and Wehman (1981) describe the experience of these early university-based programs.

Another national program, which can serve as a supplement to transitional-employment programs, is the Targeted Jobs Tax Credit. This program provides a tax credit to employers who hire handicapped workers (or workers who have other specified employment difficulties). The credit totals as much as half of a worker's wages in his or her first year of employment and a quarter of the wages in the second year.[1] Many transitional-employment programs have used this credit and the ARC-US OJT program as inducements to employers to hire program participants.

Finally, the Comprehensive Employment and Training Act (CETA) identified handicapped persons as a special population and provided some mentally retarded persons with a variety of training and work experiences. In fact, several of the transitional-employment programs operated by foundations and nonprofit organizations used CETA funding to support their efforts. To some extent, this type of support has continued under the Job Training Partnership Act (JTPA), which designates handicapped individuals as a special population.[2]

Evidence on the potential value of transitional-employment programs also comes from some programs that have enrolled populations other than mentally retarded persons. The national Supported Work demonstration was particularly influential in shaping the STETS demonstration. That demonstration, which was targeted toward long-term AFDC recipients, ex-addicts, ex-offenders, and disadvantaged youths, found that the effectiveness of transitional employment varied by target group: the AFDC recipients, who had the lowest prospects of securing jobs in the absence of the intervention but who were highly motivated, experienced long-run improvements in employment, but the labor-market impacts for the other, less disadvantaged, groups were small or zero (see MDRC, 1980, or Hollister, Kemper, and Maynard, 1984). This finding influenced MDRC in its decision to develop the STETS demonstration: because mentally retarded workers were severely disadvantaged but highly motivated, the expectation was that transitional services could be a particularly effective vehicle for improving their labor-market opportunities and actual employment.

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[1] The ceiling on the credit and the fact that it reduces the employee's regular deduction for employee wages imply that the credit is worth between $900 and $2,580 per worker in the first year (depending on the employer's tax bracket). It is reduced by about one-half in the second year of employment.

[2] In some cases, transitional-employment program operators have reported that the emphasis of JTPA on local decision-making facilitates their efforts to secure funding.

This program history suggests that transitional-employment programs are feasible, and that they may provide an effective vehicle for enabling mentally retarded young adults to become economically and socially self-sufficient. However, because of the general lack of rigorous evaluations of prior programs, it is difficult to judge their merits or to assess their appropriate role in public policy directed toward mentally retarded persons. While all previous programs have been successful at placing some mentally retarded individuals in competitive jobs, the costs of doing so have not been generally established. Moreover, it is unclear what would have happened to clients in the absence of these programs. Thus, while apparently successful, the magnitude of the program effects and the cost-effectiveness of the programs themselves are unknown.

Despite the paucity of rigorous evaluations, some in-depth studies have suggested that transitional-employment programs for mentally retarded persons may be cost-effective. In particular, the work of Hill and Wehman (1983) and Hill et al. (1985) suggests that Project Employability (a transitional-employment program for moderately and severely retarded persons) generated benefits that exceeded costs by a substantial margin. In addition, program assessments made in Washington and Massachusetts have convinced those states to expand their transitional-employment efforts (see, in particular, O'Neill and Associates, 1983, for a discussion of activities in the state of Washington). In addition, the Social Security Administration was sufficiently confident in this approach that it has funded a demonstration to test the efficacy of transitional employment for mentally retarded SSI recipients.

II. THE STETS DEMONSTRATION

In view of the evidence from a number of small demonstration and program efforts which suggests that transitional-employment and training services can benefit mentally retarded young adults, the Manpower Demonstration Research Corporation (MDRC) assumed the task of designing and operating a demonstration to determine (1) how a set of employment-oriented transitional services could operate in practice on a scale previously attempted only by one organization (Job Path in New York City), and (2) how effective those services could be in placing the target population in unsubsidized employment. This chapter presents an overview of the demonstration design as it pertains to the impact and benefit-cost evaluations.[1] It then describes the research issues that underlie this report and the data collection design necessary to evaluate those issues.

A. THE DEMONSTRATION MODEL

The natural tensions that arise from underlying programmatic and policy concerns affected the ultimate design of the STETS demonstration. On the one hand, efficient tests of particular program models necessitate that demonstration programs be designed and implemented with close adherence to specific implementation guidelines. On the other hand, a demonstration design that calls for implementing specific program models in a rigid manner precludes gaining substantive knowledge from affording program operators a reasonable degree of autonomy in designing and operating programs that meet the established guidelines and objectives. The issues underlying the STETS demonstration were such that the optimal design fell somewhere between a design whose purpose was to test very specific program models (which would enable one to learn a great deal about a very limited set of program and policy questions) and a design whose purpose was to examine a variety of program models and implementation strategies designed to address established policy objectives (which would permit a less in-depth analysis of a much broader set of programmatic and operational options).

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[1] Detailed information on the design and implementation of the STETS demonstration program is contained in MDRC's own implementation report (Riccio and Price, 1984).

This intermediate strategy was suggested by three factors. First, the overall objective of the program was clear from the start--to place mentally retarded young adults in competitive employment. Second, the wealth of previous experience with mentally retarded individuals and other groups who experience persistent employment problems suggested an employment strategy based on transitional services--services that might include social and world-of-work training, as well as job-skills training. And, third, as a result of funding options, future programs are likely to be operated by local service organizations that tailor centrally developed plans to local needs and service availability. Thus, the approach adopted by MDRC was to specify the STETS program in terms of (1) the definition of program phases, their general content, and their time parameters, (2) project selection criteria, and (3) client eligibility criteria. Within these constraints and guidelines, the individual projects had some latitude to implement their programs as they saw appropriate.

1. Program Phases[1]

The program experience for clients consisted of three sequential phases: (1) assessment and work-readiness training; (2) transitional jobs; and (3) follow-up support services.

Phase 1: Assessment and Work-Readiness Training. Phase 1 of the program combined training and support services in a low-stress environment, the goal of which was to help participants begin to develop the basic work habits, skills, and attitudes necessary for placement into more demanding work settings. This preliminary stage, which was limited to 500 hours of paid employment, occurred in either a sheltered or a nonsheltered work setting, but, in all cases, the participant's wages were paid by the project.

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[1] This section and the next borrows freely from Riccio and Price (1984), pp. 11-12 and pp. 21-23.

During Phase 1, participants were to engage in at least 20 hours of productive work weekly, with additional time spent, as necessary, in other activities that would enable them to develop the behavior and knowledge required in Phase 2 employment positions. Based on these activities, program operators were able to assess the abilities and interests of mentally retarded workers, a process which is considered essential for identifying necessary support and training services. The activities also provided information to help staff place participants into appropriate jobs in subsequent phases of the program.

Phase 2: Transitional Jobs. At all sites, Phase 2 was essentially a period of "on-the-job" training in local firms and agencies. During this stage, participants were placed in nonsheltered positions that required at least 30 hours of work per week, and in which, over time, the levels of stress and responsibility were to approach those found in competitive jobs. In developing Phase 2 job slots, programs emphasized positions that would lead to regular employment. Wages were paid by either the project or the employers and, in many cases, by a combination of the two. The STETS program provided workers in Phase 2 with counseling and other support services, and it helped the line supervisors at the host company conduct the training and necessary monitoring activities.

Because STETS was to provide a relatively quick transition to employment, MDRC guidelines limited paid participation to 12 months during Phases 1 and 2 combined. To compensate for periods of inactivity caused by participant- or program-related problems, MDRC guidelines allowed participation to span a 15-month calendar period.

Phase 3: Postplacement Support Services. The third phase of program participation began after participants had secured regular employment. According to MDRC guidelines, Phase 3 began when each of the following conditions was met:

1. The employer was not receiving a financial subsidy from the program.
2. The program had substantially reduced counseling and other services to both the participant and the employer.

3. The staff and the employer considered the participant to be a regular member of the workforce, rather than a trainee.

The purpose of this phase of program services was to ensure an orderly transition to work by tracking the progress of participants, by providing up to six months of postplacement support services, and, if necessary, by developing linkages with other local service agencies.

2. Project Selection Criteria

MDRC selected local program operators on the basis of two types of considerations--diversity and capability. In terms of diversity, the major elements included geography and the nature of the local project organization. Geographic diversity entailed selecting sites that represented a variety of urban settings.[1] To attain organizational diversity, MDRC sought projects from among traditional and modified sheltered workshops, state agencies, and nonprofit training programs.

In terms of project capability, MDRC considered four elements. First, to ensure that the demonstration would begin quickly, MDRC sought projects that had already successfully operated employment-related programs for mentally retarded or other handicapped individuals. Second, to ensure that each project could reach a goal of 40 to 55 slots, MDRC considered only those operators who could provide a solid organizational structure and who had established concrete relationships with potential referral agencies and service providers. Third, MDRC required that program operators demonstrate that they could generate a large portion of local operating costs independently of MDRC, thereby demonstrating both their commitment to the demonstration and their capacity to continue operations after the end of the demonstration. Fourth, the communities in which the projects were to operate were to be large enough to ensure an adequate flow of program applicants without significantly affecting the opportunities available to persons who would not participate in the demonstration.

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[1] Although rural settings were considered, they were ultimately ruled out due to population-size limitations.

This last consideration--an adequate community size--has important implications in terms of the research effort. In order to assess the STETS intervention accurately, the introduction of the program into the community should not have altered the local service and employment environment.[1] If the STETS program induced other programs to change the services they provided or the persons they served, or if STETS changed the local labor market, then the impacts of the program on clients would be masked by the effect of a changing environment. Selecting adequately sized service delivery areas and labor markets relative to the modest program sizes helped minimize the likelihood of such adverse population affects and the concomitant analytical bias.

3. Client Eligibility Criteria

MDRC established eligibility criteria for two seemingly competitive purposes. The first and obvious one was to limit program participation to those who could potentially benefit from program services. The second and less obvious purpose was actually to encourage projects to recruit and enroll a broad range of clients in order to provide an adequate information base for examining the suitability of STETS for a diverse population. Thus, rather broad eligibility criteria were necessary to meet these two goals. Accordingly, each client was to meet the following criteria:

- **Age between 18 and 24, inclusive.** This group was chosen to enable the program to focus on young adults who were preparing for or were undergoing the transition from school to work or other activities.

- **Mental retardation in the moderate, mild, or lower borderline range.**[2] This criterion was indicated by an IQ score of between 40 and 80, or by other verifiable measures of retardation. Specifically because IQ scores have been challenged as a valid measure of employability, special efforts were made to recruit applicants whose IQs were in the lower ranges, to ensure that the effectiveness of the program for this group could adequately be tested.

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[1] As we discuss in Section II.B, it is particularly important that the environment faced by the control group remain relatively unchanged.

[2] Moderate, mild, and borderline mental retardation are defined by IQ scores in the respective ranges of 36 to 51, 52 to 68, and 69 to 80.

- **No unsubsidized full-time employment of six or more months in the two years preceding intake, and no unsubsidized employment of more than 10 hours per week at the time of intake into the program.** This criterion was established to limit enrollment to persons who would be likely to need the intensive employment services envisioned in the model.

- **No secondary disability that would make on-the-job training for competitive employment impractical.** While the demonstration was designed to test the effectiveness of the program for a disabled population, it was recognized that some individuals would have secondary disabilities of such severity that these individuals could not be expected to work independently in a regular job. While the projects were required to make such determinations, they were encouraged to apply this standard only in exceptional cases.

Projects were encouraged to recruit and enroll a broad range of clients in three specific ways. As we just noted, they were encouraged to apply the standard on secondary disability only in exceptional cases. Essentially, projects were expected to work with clients who had secondary disabilities, if those disabilities did not make the training and placement impossible or unreasonably difficult for the client or the project. In addition, projects were encouraged to enroll relatively more lower-functioning clients than they might have otherwise. Specifically, as their goal, half of their total population of clients were to have IQ scores in the range of 40 to 60. Finally, projects were discouraged from enrolling only those whom they considered in advance to have the highest likelihood of success in the program (i.e., they were discouraged from "creaming"). Such individuals may also have been those who would have been more successful outside of the program, and a key purpose of the demonstration was to determine for whom the program would have the greatest effects.

B. RESEARCH ISSUES AND EXPERIMENTAL DESIGN

As described earlier, this report focuses on the impacts of STETS on experimental group members (i.e., program participants) and on the program's benefits relative to its costs. To be judged effective, a transitional-employment program for mentally retarded individuals must be evaluated along the same dimensions as other employment and training programs. In addition, transitional-employment programs have important social objectives that must be considered. However, because the evaluation can be designed to address only a limited number of issues efficiently, it is necessary to consider previous experience and policy interests to determine the issues and hypotheses that both are policy-relevant and can be analyzed within the context of the demonstration. (Chapter I reviews many of the considerations that defined the research focus.) Accordingly, the STETS research plan was designed to address five basic questions:

1. Does STETS improve the labor-market performance of its participants? How and to what extent?
2. Does STETS affect the use of alternative programs by participants? Which ones?
3. Does STETS participation help individuals lead a more normal life-style?
4. In what ways do the characteristics and experiences of participants or of the program influence the effectiveness of STETS?
5. Do the benefits of STETS exceed the costs?

In order to formulate a research design, we translated these broad questions into specific hypotheses, which are discussed in Section B.3 of this chapter. However, this translation necessitated that we define the most relevant comparative alternatives to STETS and how these alternatives could be incorporated into the research design. Thus, before discussing the hypotheses, we address issues pertaining to alternative programs and services.

1. Alternatives to STETS

Most of the questions examined in the demonstration ask in one form or another whether STETS induces participants to change their behavior from what it would have been in the absence of the program. To assess this change, one must determine the actual behavior of participants and what their behavior would have been had they not been offered the opportunity to enroll in STETS.

Determining the actual behavior of participants is relatively straightforward--it can be determined from interviews with participants or their proxies. Determining the alternative situation is more difficult. Individuals who entered STETS were those who were seeking to enter the regular labor force. Accordingly, they were in the referral/case-management network, which includes the school systems, sheltered workshops, departments of vocational rehabilitation, and similar agencies. If participants had not entered STETS, most would have continued or enrolled in the traditional types of programs offered by these agencies. Of course, some individuals might have decided not to enter any formal program, and would have stayed at home or sought jobs informally. Therefore, the appropriate alternative to which STETS should be compared is the mix of alternative programs that would have been used by participants in the absence of STETS.

The most straightforward way to estimate what the behavior of STETS participants would have been in the absence of the demonstration is to observe the behavior of a control group which is identical to STETS participants, but which was not given the opportunity to enroll in the demonstration program. Control group members were to resemble the participants in terms of personal characteristics and pre-STETS behavior, and they were to have had the same opportunities that STETS participants would have had in the absence of the demonstration.

Although STETS could potentially be judged relative to a counterfactual situation in which no other treatments existed, this alternative is neither practical nor particularly relevant. Because individuals who were referred to STETS were in the service delivery system, they were expected to be involved in programs or treatments in the absence of STETS; any test of program utility or worth should thus be made relative to the alternative service-delivery system that would have been replaced or complemented by STETS.

2. Experimental Design

Because STETS was introduced in the sites as a new or expanded program, its introduction was compatible with the experimental design. The basic plan was to assign eligible STETS referrals randomly to either the experimental group (which was given the opportunity to enroll in STETS) or the control group (which was not offered STETS services for the full duration of the study, but which could have used other services available in the community). The goal of this procedure was to produce two groups that would be virtually identical in terms of both observable characteristics (e.g., age, IQ, gender, and pre-STETS activities) and unobservable characteristics (e.g., motivation and ability). Some differences might still have arisen by chance, but they should be small and can reasonably be controlled for statistically in the course of the research.

Random assignment was planned as part of the sample intake process. Projects were permitted to recruit and screen applicants in any manner that was consistent with program rules. However, because random assignment was judged to be so critical to the integrity of the research design, it was implemented in a manner whereby the evaluation contractor, Mathematica Policy Research, Inc. (MPR), could control the process carefully. Essentially, when an applicant indicated his or her willingness to cooperate with the requirements of the program and the research, and when the project determined its willingness to accept the applicant, the intake worker called MPR to verify that the individual was a first-time applicant and to receive the applicant's assignment to experimental- or control-group status. The full order of assignments was generated randomly for each project prior to the start of the demonstration. These lists were not shared with the projects but were instead maintained by MPR, which assigned the statuses (experimental or control) to applicants in the order in which applicants were determined by projects to be appropriate for the STETS program.[1]

3. Hypotheses of Program Effects

The impact analysis consists of four specific areas into which the various outcomes of interest fall logically: (1) labor-market behavior, (2) training and schooling, (3) public transfer and other program use, and (4) economic status, independence, and life-style. In addition, the benefit-cost analysis considers STETS from the perspective of an investment.

Labor Market Behavior. The primary labor-market outcomes of interest are employment, earnings, and hours worked. Other outcomes include job characteristics and job tenure. Increased employment in unsubsidized, competitive jobs was the primary objective of STETS: the demonstration was based on the hypothesis that STETS would enhance the ability of participants to obtain and hold jobs in the regular labor market. Conversely, it was hypothesized that STETS would prompt participants to rely less on sheltered workshops, activity centers, and training jobs (in the postprogram period).

STETS should also have an impact on earnings and work hours. It was hypothesized that the training and work experience of STETS would allow participants to work more hours and earn more money. Wage rates may also be affected by STETS, but the direction of the effect is less clear. Overall, individuals should have been able to perform more capably in jobs, thereby earning higher wage rates. However, some marginal workers (individuals who would not have been able to work in competitive jobs without STETS services) were also expected to be able to obtain jobs subsequent to their STETS experience. Therefore, for experimentals as a group, the higher wages of more able individuals who performed more capably in jobs would tend to be offset by the lower wages associated with some less able individuals' obtaining jobs for the first time.

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[1] This procedure was based on the system developed by MPR for the national Supported Work demonstration.

Also important in assessing STETS are job characteristics. Other than wage rates and hours, occupation is the primary characteristic that can be observed. As with wage rates, it was hypothesized that improvements in area employment rates would increase the likelihood that less able individuals would obtain competitive jobs for the first time. To the extent that this likelihood occurs and that the jobs obtained by these less able individuals tend to be in low-skill occupations, program-induced improvements in job quality for more able individuals may be masked. STETS was also hypothesized to improve the lives of its participants by improving their ability to hold onto and to develop in the jobs they obtained. Although data on continuous job-holding are not available in this study (for reasons cited below), we did hypothesize that, for those who held competitive jobs, participants would have a higher degree of job retention than controls--a hypothesis that can be tested using point-in-time data. Similarly, for those who lost a job, participants were hypothesized to be more likely than controls to find another.

Training and Schooling. Central to the evaluation is the question of how STETS affects the use of training programs and schools, programs that can lead to self-sufficiency. Almost by definition, STETS participation should induce a short-term increase in the use of training programs. However, the use of training programs other than STETS should decrease. In the longer-run (i.e., beyond the period of STETS eligibility), STETS was hypothesized to reduce the use of all training programs as a result of increasing employment. The effect on school attendance was expected to be consistent over time: participants were hypothesized to be less likely to attend school, both during and after program participation.

Public Transfer Use. A large portion of the population eligible for STETS was also eligible for one or another form of public transfers. The main program from which mentally retarded young adults may draw benefits is Supplemental Security Income (SSI). Others may draw benefits under Old Age, Survivors, and Disability Insurance, commonly referred to as Social Security Disability Insurance (SSDI). In general, individuals who are covered by SSI are also eligible for Medicaid or the state alternative, and those who are covered by SSDI are eligible for Medicare. Individuals and their families may also be eligible for welfare--Aid to Families with Dependent Children (AFDC), general assistance, food stamps, or subsidized housing.

The strongest hypothesis was that STETS participation would reduce dependence on SSI and SSDI, as well as on Medicaid and Medicare, through its effects on employment and earnings. However, the SSI/SSDI effects should have occurred with a lag, because of delays in reporting and in benefit adjustments and because of provisions in the regulations that provide some protection to recipients. A similar hypothesis was made for welfare and food stamp receipt. However, this hypothesis is weaker because entitlement under these forms of transfers is usually based on family or household units, and the financial well-being of a STETS participant may not have substantially changed the financial well-being of the family. Finally, the effect on subsidized housing is ambiguous: participants were hypothesized to become more independent in their living arrangements, but this could have been accomplished, for example, through a move from their families to some form of subsidized housing, or from subsidized housing to an independent living situation.

Economic Status, Independence, and Life-Style. STETS seeks to make participants more self-sufficient in an effort to enable them to lead life-styles that are more compatible with the general population. The *economic status* of participants was expected to improve, with earnings gains being greater than transfer losses. Other effects were expected to occur over the long term as earnings gains were realized: STETS was hypothesized to lead to *living arrangements*, *financial management skills*, *social behavior*, and *service use* that would be more compatible with the general population.

STETS as an Investment. As an *investment*, STETS can be viewed from various perspectives, including participants, nonparticipants (i.e., all others in society), and society as a whole. From the participant perspective, STETS was hypothesized to generate benefits that exceed costs. From the nonparticipant perspective, the hypothesized effect is ambiguous: program operating costs would be offset to an unknown degree by savings in many other areas. From the social perspective, STETS was generally hypothesized to generate benefits that exceed costs, but the hypothesis is somewhat ambiguous. Complicating the test of these hypotheses is the fact that the program was likely to generate substantial social and psychological benefits (and some costs) that could not be valued, and that would be considered but not valued explicitly in analyzing STETS as an investment.

Subgroup Analysis. The preceding sections have identified the basic hypotheses for the economic and social outcomes of interest. In addition to testing these basic hypotheses, it is also useful to examine the extent to which the effects of the program differed among sample subgroups and program features, since information on these differences will be helpful in planning and targeting future transitional-employment programs. With exceptions, the hypotheses pertaining to differences among subgroups are ambiguous. They are also not necessarily consistent across outcome variables. Therefore, in this section, we present a brief overview of the subgroups of interest rather than speculate on the specific hypotheses.

The subgroups that are considered fall into the following categories:

- Demographic characteristics
- Personal characteristics
- Previous experience and attainment
- Program features and characteristics

The first category, demographic characteristics, includes such factors as age, race or ethnicity, and gender. Previous studies of disadvantaged groups (although typically not mentally retarded individuals) often show that program effectiveness can vary along such dimensions. Such variation can be caused by many factors, including actual social or cultural differences among people or differences perceived by potential employers.

The second category, personal characteristics, pertains to more individual-specific traits, such as intellectual ability, causes of retardation, and handicaps other than retardation. Intellectual ability is measured in this study by IQ scores, and the broad range of scores permitted by the eligibility criteria suggest that it may be possible to identify reasonably distinct subgroups. Documenting the other two characteristics is more difficult within the context of the research data collection effort, and the subgroups will undoubtedly be distinguished imperfectly. In fact, since the eligibility criteria imposed limits on the nature of secondary handicaps, the variation in terms of this characteristic is likely to be limited.

The third category, the activities and experience of sample members in the period before they enrolled in the program, constitute a collection of factors that reflect both the obvious direct experiences and whatever personal characteristics cannot be observed directly. These variables include primarily baseline or pre-baseline measures of the various outcome variables.

While the fourth category, program features and characteristics, would appear to include obvious candidates that would condition program effectiveness, it presents two serious problems. In terms of the variables that describe the programs themselves, the distinctions among them cannot easily be quantified, nor do only five projects provide enough variation to distinguish among program features. Therefore, site is the only variable of this type that can be incorporated in the analysis. Of course, any program effects that are associated with site might also reflect differences among the local areas (i.e., in terms of job opportunities, alternative services, etc.). In terms of the variables that reflect the specific services provided to individual STETS participants, the fact that services were assigned at least to some degree on the basis of need rather than on the basis of random selection introduces a selectivity problem into the analysis that might produce biased estimates of subgroup effects. Despite this problem, we selectively consider a few key subgroup classifications of this type in analyzing the impacts of the program on earnings, taking precautions as best we can to minimize selectivity bias in the impact estimates.

C. DATA COLLECTION

Testing the various research hypotheses requires data on the activities and experience of experimentals and controls from the time they first came into contact with the program and were randomized into the research sample to a point 22 months later. Thus, the requisite data include information on labor-market activities, participation in training and schooling programs, the receipt of transfer payments, the use of support services, and other activities pertaining to self-sufficiency, as well as information on important demographic and personal characteristics. These data had to be collected in a standardized manner for all sample members (whether in the experimental or control group), over time at intervals that would appropriately capture the effects of the intervention and key program events, and consistently across the five demonstration sites.

Evaluations of many employment programs have relied on self-reported data collected in interviews. The difficulties experienced by STETS sample members in terms of physical and cognitive functioning and communication skills raised serious concerns about the quality of self-reported data. However, alternative sources of data--primarily proxy respondents (such as parents, guardians, or counselors) and administrative records from service agencies or public assistance programs--have their own limitations in terms of comprehensiveness and accuracy. After a careful consideration of the issues, we developed a data collection strategy that relied on multiple sources, with the primary source being self-reports from sample members. However, these data were supplemented by data from proxy respondents that were collected when sample member data were missing or inconsistent and by records data that were collected from referral and other agencies and the STETS projects, which provided critical baseline measures and details on service utilization.

The remainder of this section briefly discusses the data collection design, and it reviews several important methodological and fielding issues. Because of the importance and complexity of the data collection issues, the topic is considered in more detail in Appendix B.

The data required for the STETS evaluation were to be collected through an integrated system that included the following:

- Interviews with the sample members and, as necessary, with proxy respondents
- Corroborating information provided by community service agencies with which the sample members had contact and that were mentioned during the interviews
- Background information on sample members collected by STETS project staff as part of the intake process
- STETS program participation data on all experimental group members
- Information on program costs collected from the demonstration accounting systems
- Observations of the STETS work activities of a sample of experimental group members

1. The Sample Member Survey

As noted above, in-person interviews that were conducted with the sample members provided the majority of data for the STETS evaluation. The survey design that was adopted reflects a compromise between (1) obtaining data at key points in time relative to the receipt of program services and maximizing the length of the follow-up period, and (2) conducting the evaluation within a fixed budget. Under this design, interviews were scheduled to be administered at several key points in time relative to program application:

- Immediately after random assignment to the experimental or control group (the baseline interview)
- At a point when the majority of experimental group members were still actively participating in the STETS project (the 6-month interview, conducted approximately 6 months after an individual's random assignment)
- At a point when most experimental group members were no longer receiving STETS services (the 15-month interview)

- At a point well beyond the end of the demonstration program services for all experimental group members (the 22-month interview)[1]

All participant and control group members were scheduled to receive the baseline, 15-, and 22-month surveys. However, due to resource constraints, only a randomly selected two-thirds of the full sample were scheduled to receive the 6-month interview, which provides data primarily for estimating in-program effects.

All interviews collected point-in-time data on employment, job training, and schooling; on involvement in life-skills training, recreational activities, counseling, and transportation assistance programs; on the receipt of cash and in-kind transfers; and on living arrangements and other measures of independence. These interviews were designed to collect quality information relative to the time of each interview (point-in-time data) because recall problems for many mentally retarded respondents precluded obtaining "time-line" data, as are often collected for studies of this type. Point-in-time data are less than ideal for the impact analysis, and they necessitate conducting some extrapolation between points-in-time for purposes of the benefit-cost analysis; nonetheless, they do enable us to obtain unbiased estimates of treatment effects at critical points in time.

2. The Proxy Survey

When the completed sample member interview contained missing or inconsistent data on specified, critical items, proxy respondents were identified and interviewed, with the written consent of the primary respondent. The proxy respondent was selected in the following order of priority:

1. A live-in parent or relative who provided help with financial management

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[1] Although an initial goal had been to collect three years of follow-up data, funding limitations precluded such an effort.

2. Any other person who provided help with financial management

3. A live-in parent or relative who provided no help with financial management

4. A social worker or caseworker

5. Someone whom the sample member indicated was generally knowledgeable

The interview that was designed for proxy respondents was identical in content to the interview designed for the sample member.

3. Service Agency Data

Community service providers, employers, and residential service agencies were identified during the interviews with primary and proxy respondents. Each organization that was not known in advance to be a private employer was contacted to determine whether it was in fact a service agency, and to collect general information on the nature and mix of services to corroborate the reports of sample members on the services they received. The goal was to collect general data from these organizations, and not to identify STETS sample members specifically or to attempt to collect individual-level service data.

4. Application/Enrollment Data

To provide a common set of data for all sample members for a point prior to random assignment, an application/enrollment (A/E) form was developed for project screening and intake staff. The forms summarized information from a variety of sources, including the applicant, parents or guardians, referral agencies, and other agencies. The form requested information on basic demographic and personal characteristics, as well as on previous experience pertaining directly to the primary outcomes of interest in the impact analysis. In part, the A/E form collected data that could not be obtained in the baseline interview, which was administered after the experimental group members had become involved in the program, when perceptions of preprogram behavior may have been distorted.

5. STETS Participation Data

MDRC maintained a Management Information System (MIS) for the STETS projects. Information on each experimental group member was provided on a monthly basis to the MIS database during the period of his or her participation in the STETS demonstration. This information included the individual's current status in the project, placement data on training and permanent jobs, reasons for changes in program status, the number of days actively involved in STETS, and the hours scheduled and actually spent on training jobs and in other demonstration activities.

6. Cost Data

To monitor the projects and to provide cost data for the analysis, MDRC required that the projects account for their demonstration expenditures. Projects were to record their expenditures for program management, training, and other services to clients. They were also required to maintain records of payments to clients while they were in Phase 1 or Phase 2 activities, including wage payments made directly by employers to clients. These data were reported monthly on standard forms to MDRC during the course of the demonstration.

7. Work Activity Observations

As called for in the data collection plan, observations of the work activities of a subsample of 40 randomly selected participants were conducted. These observations were designed to collect information on the value of output produced by participants in their Phase 1 and Phase 2 jobs. For each participant studied, we interviewed the direct work supervisor in order to identify the following:

- The output produced by the participant
- The wages paid to the participant by the employer
- The cost that would have been incurred by the employer to produce the output had the participant not been hired

- The additional supervisory and production costs incurred to produce the output due to the on-the-job training nature of the participant's job

This information was subsequently integrated into the benefit-cost analysis to value the average in-program output per experimental, a program benefit that has proved to be important in previous transitional-employment programs.

III. IMPLEMENTATION OF THE DEMONSTRATION AND RESEARCH PLANS

In Chapter II, we described the demonstration and research plans up to the point at which they were actually implemented. This chapter provides background information on the environment in which those plans were implemented, as well as on the outcomes of both the sample selection and data collection plans. We begin by providing brief descriptions of the local projects selected by MDRC to operate STETS programs under its direction. In Section B, we present a more detailed discussion of the sample selection process--specifically, recruitment, sample size, and the implications of sample size for the research plan--and of the characteristics of those who were selected to participate in the STETS demonstration. In Section C, we discuss the response rates to the surveys and the quality of our data. We conclude the chapter by providing a brief review of the analytic methodologies that will help form the foundation for the various impact analyses and the benefit-cost analysis which follow in the remainder of the report.

A. THE PROGRAM ENVIRONMENT

Operators in five cities--Cincinnati, Ohio; Los Angeles, California; New York, New York; St. Paul, Minnesota; and Tucson, Arizona--were selected to implement STETS programs. The brief descriptions that follow help highlight one of the project-selection criteria discussed in Chapter II--namely, a diversity in terms of project capabilities.[1]

1. The STETS Project Organizations

STAR Center, an acronym for Services, Training, and Rehabilitation, is the largest sheltered workshop in Cincinnati, Ohio. It is a division of the Workshops for Retarded Citizens, Inc., which is also the parent organization of a job-placement service for the disabled, the Joy Center. STAR provides vocational evaluation, skills and job-readiness training, and sheltered employment for handicapped persons, with a focus on mentally retarded and emotionally disturbed individuals. In recent years, STAR has participated in a Projects With Industry program that offers training positions in Cincinnati's largest hospital. Although STAR had previously attempted to prepare its clients for unsheltered positions, its efforts to achieve this goal increased considerably under the STETS program.

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[1] The remainder of this section borrows freely from Riccio and Price (1984, pp. 13-16).

ADEPT, an acronym for Assisting the Disabled with Employment, Placement, and Training, operated the STETS project in Los Angeles, California, in cooperation with the California Institute on Human Services (CIHS). ADEPT is a nonprofit agency that offers a variety of employment services to mentally retarded workers, although most of its efforts prior to STETS had been devoted to finding job placements for physically handicapped persons. CIHS exercised administrative control over the Los Angeles STETS project, while ADEPT provided the direct services to participants. The STETS program in Los Angeles operated in two separate ADEPT offices; the main office was located in Panorama City in the southern part of the San Fernando Valley, and a satellite office was housed in an ADEPT branch in downtown Los Angeles.

Job Path, in New York City, was the only local operator with prior experience in transitional-employment programs for a mentally retarded population. While both Job Path and STETS used similar techniques, some distinctions are worth noting. Job Path serves mentally retarded persons of all ages (the average age of participants was 28 years during the implementation phase of the STETS demonstration); STETS was targeted toward 18- to 24-year-olds, and thus effected an increased emphasis on youth in the population served by the organization. Job Path also differed from STETS in terms of the length of participation in each program phase; specifically, STETS was designed to provide a quicker transition to employment.

Minnesota Diversified Industries (MDI), in St. Paul, Minnesota, is a private, nonprofit corporation that describes itself as an "affirmative industry." While its facility resembles a sheltered workshop in some respects, MDI appears to emphasize more sophisticated production techniques than are typically found in workshops. It also has a more varied workforce than is typical of workshops: most, but not all, of its workers are handicapped, and mental retardation is only one of the handicaps represented. Before its association with STETS, MDI had experimented with a few smaller-scale efforts to place mentally retarded workers into unsubsidized jobs with local firms and agencies.

The Department of Economic Security, Division of Developmental Disabilities (DDD), in Tucson, Arizona, was the only government agency to operate a STETS project. The agency offers a variety of residential, vocational, and support services to developmentally handicapped individuals of all ages. Many of its clients receive employment and training in a range of settings, including work activity centers, sheltered workshops, and, increasingly, competitive employment. DDD does not itself offer training services, but typically coordinates services provided by other organizations and refers clients to them. Developmentally handicapped individuals are assigned to a DDD "case manager," who prepares an individual development plan that specifies the client's needs and goals and defines the strategies for achieving them. In STETS, DDD assumed more direct responsibility for providing employment and training services to its clients.

2. Local Service Environment

A comparison of the service environment of the sites in which the demonstration was implemented with the service environment of other possible demonstration sites provides important information for interpreting the analytical results. While it is difficult to draw any sort of quantitative comparison, it would appear that each area in which STETS was implemented was relatively service-rich. That is, although we were not aware of any other large-scale transitional-employment programs that were operating in any of the sites, mentally retarded individuals were served by case-management programs, workshops and activity centers, the schools, and a variety of other programs. Furthermore, all five sites were very receptive to the demonstration program, as shown at the proposal stage by their commitments for referrals, local funds, and complementary services, and by their assessments of employment prospects.

To the extent that the receptivity of the local environment facilitated implementing the program, it may have boosted program effectiveness beyond what could have been expected in other settings. However, the potential net gains from similar transitional-employment programs would be greater among programs that were operated in sites that offered fewer alternative services to its mentally retarded citizens. The primary obstacles to achieving this greater impact potential are the greater start-up problems that might occur as a result of both the relative inexperience of the site in serving this client group and the potentially less favorable attitudes of relevant community members toward increasing the level of employment and the degree of economic independence of mentally retarded young adults.

B. THE RESEARCH SAMPLE

The sample of persons who were enrolled in the demonstration reflects the outreach, recruiting, and screening activities of the five demonstration projects. Thus, before describing the sample of individuals studied, we review the sample selection process. We then describe the size of the sample (and its implications for the research), the characteristics and experience of experimentals prior to enrolling in STETS, and, finally, the validity of the control group.

1. Sample Recruitment and Selection

As we indicated in Chapter II, program operators were permitted to develop their own methods for recruiting applicants into their projects; however, in all cases, it was essential that referrals be made from agencies that could verify that the applicants were mentally retarded. By far the most important referral sources were the vocational rehabilitation agencies and the school systems, but referrals also came from workshops, agencies for the developmentally handicapped, and a variety of other governmental and private organizations. Some projects also relied on the news media to gain general acceptance for the program, but not necessarily to attract applicants directly. Thus, virtually all of the individuals recruited for STETS were in the service-delivery system and were referred to the program by case managers.

2. Sample Size

New York (Job Path) was the first STETS project to accept enrollees on a trial basis, in October 1981. In November 1981, New York, Cincinnati (Star Center), and Tucson (DDD) began the intake process for individuals who became part of the research sample. These projects were joined by Los Angeles (ADEPT) and St. Paul (MDI) in March 1982. All projects phased out intake by December 1982. The final sample eligible for inclusion in the impact and benefit-cost analyses consisted of 437 individuals who completed the baseline and one or more follow-up interviews--of these, 226 were randomly assigned to the experimental group, and the other 211 were randomly assigned to the control group. In total, up to 287 of these sample members have been included in analyses of the 6-month impacts; up to 415 have been included in analyses of the 15-month impacts; and up to 403 have been included in analyses of the 22-month impacts. A total of 58 program participants were excluded from the analysis--48 who enrolled prior to random assignment, 2 who failed to complete the baseline interview, and 8 who completed the baseline but none of the follow-up interviews.

The final research sample size is less than half of what was originally planned for the study, due largely to funding constraints. This reduction in sample size had implications in terms of the overall strength of the evaluation results. In particular, the sample size affected both the statistical power of the impact analysis and the confidence level of the results. By statistical power, we mean the likelihood that the analysis will detect the occurrence of program effects if STETS did indeed have such effects; the confidence level of the results refers to the likelihood that we will not wrongly conclude that the program induced particular effects when, in fact, the program had no impact.

Although somewhat arbitrarily, a 90 percent level of confidence and a 70 to 90 percent level of power are commonly adopted as minimum standards. By applying these standards to the STETS sample, one may conclude that there is a 90 percent probability that the estimated impacts of the program will be statistically significant at the 90 percent level of confidence if the program increases job-holding, hours of employment, and earnings on regular jobs by, for example, 66 to 75 percent; there is a 70 percent probability that 50 percent increases in employment outcomes will be detected with a 90 percent level of confidence.[1] While effects of this order of magnitude could clearly be realized, it must be remembered that achieving these very large impacts is in no way a criterion for judging the success of the program. More modest impacts could prove to be sufficient to justify this type of program intervention. Thus, although the sample size seems adequate for estimating overall program impacts, it is important to consider the overall pattern of the impact estimates.

Relatedly, it is important to recognize the substantially lower statistical power of the impact results for sample subgroups. For example, the probability of detecting program impacts as large as 50 percent of the control group means drops from 90 percent based on the full sample (with a 90 percent level of confidence) to 66 percent based on a subgroup which consists only of one-half of the sample, and to 52 percent for a subgroup which consists only of one-third of the sample. Thus, especially in our interpretation of the subgroup results, it is important to recognize the severe sample size constraints and to rely heavily on informed judgments based on the patterns and sizes of the estimated impacts.

While the minimum detectable differences that were calculated for the stated levels of precision are large relative to the expected levels of activities in the absence of STETS, we must remember that these base levels of activities (i.e., the levels found among the control group) are quite limited, and we have every reason to expect large experimental effects.[2]

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[1] Had we achieved the initially proposed sample size, which was about double the size of the one used, we could have had a 99 percent probability of detecting effects in the range of 47 to 53 percent of the control group means and a 70 percent probability of detecting effects as small as one-third of the control-group means.

[2] Smaller effects are still important to the evaluation of STETS; however, they are measured at even lower levels of statistical precision.

The major area of concern pertains to the subgroup analysis. We must be very careful how we interpret experimental effects for subgroups, since the absence of a statistically significant effect may in some cases be due to the lack of statistical power associated with the small sample size. Accordingly, for some components of the analysis in which it is important to calculate differences based on small subgroups, the analysis must be regarded as largely descriptive, and we cannot apply our usual statistical standards. Furthermore, while we will analyze experimental effects for subgroups, the sample size will not support a statistical analysis of the differences in effects among such groups: not only are the subgroup sample sizes small, but the differential effects among groups are also expected to be relatively small.

3. Characteristics of the Experimental Sample

This section describes the characteristics of the experimental group. We then consider the program experience of the experimental group and a few relevant issues pertaining to the control group.

As shown in Table III.1, the experimental sample was fairly evenly distributed across projects, with the exception of St. Paul: 20 percent of the respondents were located in Cincinnati, 22 percent in Los Angeles, 25 percent in New York, 22 percent in Tucson, and just 12 percent in St. Paul. Males constituted 58 percent of the sample. The average age at enrollment was 20 years; 34 percent were 22 years of age or older. Over half of the sample members were white, 30 percent were black, and most of the remainder were of Hispanic origin.

The experimental sample was concentrated more within the mild range of retardation than had been intended: only 12 percent of the sample were in the moderately retarded range, while 60 percent were in the mild range, and 28 percent were in the borderline range. In the sample, 39 percent suffered from secondary handicaps. Referral agencies reported a specific organic cause of retardation for 18 percent of the experimental sample members.

TABLE III.1

CHARACTERISTICS OF THE RESEARCH SAMPLE AT BASELINE, BY EXPERIMENTAL AND CONTROL GROUP STATUS

(Percent)

Characteristics	Experimental Group	Control Group	Total
Site			
Cincinnati	19.5	20.9	20.1
Los Angeles	22.1	19.9	21.1
New York	24.8	24.2	24.5
St. Paul	11.9	12.8	12.4
Tucson	21.7	22.3	22.0
Age			
Younger than 20	31.9	38.4	35.0
20 or 21	34.5	35.1	34.8
22 or older	33.6	26.5	30.2
Gender			
Male	57.5	57.3	57.4
Female	42.5	42.7	42.6
Race/Ethnicity			
White	54.0	48.8	51.5
Black	30.1	35.1	32.5
Hispanic	13.3	14.2	13.7
Other	2.7	1.9	2.3
IQ Level			
Borderline	27.9	30.3	29.1
Mild	60.2	60.2	60.2
Moderate	11.9	9.5	10.8
Secondary Handicap	38.9	32.2	35.7
Organic Cause of Retardation	18.4	15.2	16.8
Raised by Working Parent(s)/Guardians	74.2	70.1	72.2
Parent(s) Held "White Collar" Job	35.1	31.8	33.5
Any Benefactor	28.3	30.8	29.5
Receipt of Transfers[a]			
SSI or SSDI	33.2	33.5	33.3
Any cash transfers[b]	48.0	53.6	50.7
Medicare/Medicaid	31.4	35.7	33.5
Any transfers[c]	63.5	67.6	65.5
Independent Financial Management Skills	28.8	25.2	27.1
Living Arrangement			
Living with parents	78.8	84.8	81.7
Living in group home[d]	6.2	6.6	6.4
Living semi-independently	4.9	2.4	3.7
Living independently	10.2	5.7	8.0
Active in Job, Training, or School During 6 Months Prior to Referral	71.7	72.0	71.9
Activities During Two Years Prior to Enrollment (of at Least 3 Months' Duration)[a]			
Regular unsubsidized job	14.3	13.7	14.2
Any job	46.9	44.5	45.8
Job training	5.3	3.8	4.6
Attending School When Referred to STETS	28.8	36.0	30.7
Number in Sample	226	211	437

NOTE: Chi-squared and t-tests of differences in the characteristics of experimentals and controls revealed statistically significant differences in the percentage who were receiving cash transfers, the incidence of secondary disabilities, the incidence of organic causes of retardation, and school attendance at referral.

[a] The headings under this category are not mutually exclusive.

[b] Includes SSI, SSDI, welfare, and other governmental cash transfers received by or on behalf of the sample member.

[c] Includes cash transfers (SSI, SSDI, welfare, etc.), Medicare, and Medicaid received by or on behalf of the sample member, and food stamps received by the sample member's household.

[d] Includes living in an institution.

The fact that about three-quarters of the experimentals were raised by at least one parent, another relative, or a legal guardian who worked "most of the time" provides some indication of the general socioeconomic background of the sample members. White-collar jobs were held by the persons who raised 35 percent of the sample members. Another environmental factor that may affect labor-market and other aspects of success is the presence of a benefactor, an individual who helps the sample member in a variety of tangible ways. At baseline, 28 percent of the sample had a benefactor.

Even before enrolling in STETS, experimentals exhibited various levels of social and personal independence or dependence. Nearly two-thirds of the sample were receiving cash and/or in-kind transfers. Nearly half of the sample were receiving cash transfers: 33 percent were receiving SSI or Social Security Disability Insurance (SSDI); most of the remainder were receiving some form of welfare, either Aid to Families with Dependent Children or general assistance. Thirty-one percent were receiving Medicare or Medicaid or other local equivalents.

The ability to handle personal financial transactions independently (paying for purchases, paying bills, and banking) was demonstrated by 29 percent of the experimental sample at baseline. There may have been additional sample members who were capable of handling financial matters independently but who had previously lacked the opportunity to do so, as well as others who could handle only some types of transactions independently.

The availability of opportunities might also have influenced independence in living arrangement, in that 79 percent of the sample were residing with parents or foster parents at baseline. Under half of the remainder (10 percent of the sample) were living independently; 5 percent of the sample were living semi-independently, such as in supervised apartments; and 6 percent were residing in group homes and institutions. (Two percent of the sample had resided in institutions at some point within the two years prior to baseline, with less than one percent residing in institutions at baseline.) At baseline, less than one percent of the sample members were living with a spouse or their own children.

While the independence of experimental sample members at baseline was clearly limited, many were still involved in labor-market activities. According to information reported on the application/enrollment forms, 72 percent of the sample members had been active in a job, training program, or school during the six months prior to their STETS referral.[1] While 47 percent of the sample had held some type of job (including volunteer work) for at least three months during the two years before referral, only 14 percent had held a regular, unsubsidized job. Moreover, 5 percent of the sample had been enrolled in a job-training program for at least three months during the two-year period. At the time of their referral to STETS, 29 percent of the sample were attending school.

4. Program Experience of STETS Clients

Those STETS applicants who were selected to be members of the experimental group were eligible to receive program services based on the three-phase model described in Chapter II. The actual services provided to the experimental group members who are included in the research sample are summarized below.[2]

Phase 1 and 2 Experience. Phase 1 training and assessment activities were offered in two different types of settings, depending on the local program operators: in Cincinnati, St. Paul, and Tucson, Phase 1 participants worked primarily in sheltered settings; in Los Angeles and New York, they were typically placed in real work settings, usually in public agencies or nonprofit organizations. As shown in Table III.2, 92 percent of the experimental group members participated in paid employment in Phase 1, and those who participated averaged 400 hours of Phase 1 paid employment. Paid employment in Phase 1 was within the 500-hour limitation for approximately 75 percent of the sample members.

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[1] Program eligibility criteria excluded from the sample any individuals who were employed for 10 or more hours per week at the time of application.

[2] For further details on the actual provision of program services to all participants, including differences among program sites, see Riccio and Price (1984), Chapters III and V. This section draws on those chapters for specific observations about projects, although statistics have been recalculated for the specific sample available for the impact analysis.

TABLE III.2

LENGTH OF PAID EMPLOYMENT AND PROGRAM PARTICIPATION OF EXPERIMENTAL GROUP MEMBERS, BY PHASE

Characteristics	Phase 1	Phase 2	Phases 1 and 2 Combined
Hours of Paid Employment (%)			
0	8.0	33.6	7.5
1-250	21.7	16.8	12.4
251-500	44.7	20.4	12.8
501-750	21.2	17.7	18.6
751-1,000	4.0	6.6	27.0
Over 1,000	0.4	4.9	21.7
Average Hours of Paid Employment[a]	400.2	513.3	766.7
Average Days of Paid Employment[a]	74.0	78.3	129.9
Total Duration of Participation[b] (%)			
1 month or less	n.a.	n.a.	1.8
2-3 months	n.a.	n.a.	5.3
4-6 months	n.a.	n.a.	16.4
7-9 months	n.a.	n.a.	16.4
10-12 months	n.a.	n.a.	16.8
13-15 months	n.a.	n.a.	26.1
Over 15 months	n.a.	n.a.	17.3
Average Months of Participation[b]	n.a.	n.a.	10.6
Average Months of Inactive Status	n.a.	n.a.	1.3
Average Months of Inactive Status, for Those with Inactive Time	n.a.	n.a.	2.9
Number in Sample			
Paid Employment	208	150	209
Hours and Duration of Participation	n.a.	n.a.	226

NOTE: Data on paid employment were obtained from the Monthly Activity Forms in the STETS Information System; data on months of participation and inactive status were obtained from the Monthly Status Change Forms in the STETS Information System.

[a] The sample is restricted to those with paid employment.

[b] The duration of participation is measured as calendar months from random assignment through final transition or termination, including inactive time. Duration is not calculated separately for each phase, because 46 percent of the participants experienced inactive time that cannot be apportioned to the phases.

n.a. means not available, due to a substantial amount of inactive time that cannot be apportioned between Phase 1 and Phase 2.

Among the experimentals in the research sample, 66 percent participated in Phase 2 paid employment, averaging 513 hours. While all Phase 2 placements were in nonsheltered work settings, the STETS counselors had a high level of contact with the participants in most cases. During the first few weeks after a Phase 2 placement, counselors tended to devote several hours a day, several days a week to offering intensive training to participants. This counseling-provision contact was gradually reduced to several times a month.[1]

Total participation in paid employment in STETS Phase 1 and Phase 2 averaged 767 hours for members of the research sample, over a period that averaged 10.6 months from the date of random assignment to the experimental group through either the transition to a job or an education/training program or termination from STETS. However, during this period, 46 percent of the sample were placed on "inactive" status for an average of 2.9 months, and some experienced other periods when no paid employment was available to them.

As shown in Table III.3, notable differences occurred across sites in the degree to which the programs moved the STETS participants into Phase 2 activity in particular, and all sites were relatively more successful in achieving this goal for males than for females. Overall, two-thirds of all program participants entered Phase 2 employment--73 percent of the males and 57 percent of the females. However, these differences were especially notable in Cincinnati and Los Angeles, where only about half of the females held Phase 2 jobs, compared with over 80 percent of the males.

Adding to the evidence which points to the differential success of the programs for males and females is the much lower incidence of longer-term Phase 2 job-holding among females: only 19 percent of the females,

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[1] In Los Angeles, counselor work-site visits were made much less frequently in both Phases 1 and 2, averaging only once every three to four weeks.

TABLE III.3

PHASE 1 AND PHASE 2 ACTIVITIES
BY GENDER AND SITE

	Site					
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent with Phase 1 Only	34.1	38.0	19.6	59.2	30.6	33.6
Males	29.0	29.0	13.3	57.1	20.8	26.9
Females	46.1	52.6	26.9	61.5	40.0	42.7
Percent with Phase 1 Hours > 500	20.4	8.0	33.9	40.7	30.7	25.7
Males	22.6	9.7	30.0	28.6	25.0	22.3
Females	15.4	5.3	38.5	53.8	36.2	30.2
Percent with Phase 2	73.8	68.2	80.4	40.8	69.4	66.4
Males	81.0	81.0	86.7	42.9	79.2	73.1
Females	56.8	47.4	73.1	38.5	60.0	57.3
Percent with Phase 2 Hours > 500	34.1	36.0	26.8	18.5	26.5	29.2
Males	38.7	51.6	23.3	21.4	41.7	36.9
Females	23.1	10.5	30.8	15.4	12.0	18.8
Number in Sample	44	50	56	27	49	226
Males	31	31	30	14	24	130
Females	13	19	26	13	25	96

NOTE: These data are based on information obtained from the Monthly Activity Forms in the STETS Information System.

compared with 37 percent of the males, worked in Phase 2 jobs for more than 500 hours (see Table III.3).

Table III.4 describes the STETS activities of sample members as of the dates on which the point-in-time interview data were collected. As indicated by these data, the 6-month interview was administered when a large portion of the sample members were still actively participating in STETS; 68 percent were involved in Phase 1 or Phase 2 activities, and an additional 14 percent were in inactive status. The program activities of participants differed notably in the various sites at this point in time. At one extreme, 88 percent of the participants in Tucson were still active, and 73 percent of those who were active had not yet entered Phase 2 employment. In contrast, only 57 percent of the participants in Los Angeles were still active, primarily in Phase 2 employment.

By the 15-month interview, 18 percent of the experimental sample members were still active in Phase 1 or Phase 2, and 81 percent had terminated or had transitioned from the program. The one site at which a sizeable proportion of participants were still participating in Phase 1 or Phase 2 activities was Tucson--6 percent in Phase 1, and 37 percent in Phase 2.

Placement and Follow-Up Outcomes. By original design, STETS participants were to move from Phase 2 into Phase 3 when the program no longer subsidized their wages. However, because many Phase 2 jobs were in fact unsubsidized (except in New York), the distinction between phases is unclear. Phase 3 generally involved a lower level of contact between counselors and participants, although some participants encountered problems with new regular employment or moving into independent living situations, which actually created a temporary need for increased contact.

As shown in Table III.5, an average of 44 percent of the STETS participants transitioned into an unsubsidized job, with the percentages ranging from a low of 30 percent among those in St. Paul to a high of 59 percent among those in Tucson. In total, over half of the males who had participated in STETS transitioned directly into unsubsidized employment, while only a third of the women did so. Of the remaining participants,

TABLE III.4

PAID EMPLOYMENT AND PROGRAM PARTICIPATION STATUS OF EXPERIMENTAL GROUP MEMBERS, BY SITE

	Site					
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Status at 6-Month Interview Date (%)						
Active in Phase 1	31.0	6.7	13.9	50.0	72.7	33.6
Active in Phase 2	27.6	50.0	50.0	22.2	15.2	34.2
Inactive	17.2	16.7	16.7	16.7	3.0	13.7
Terminated/ transitioned	24.1	26.7	19.4	11.1	9.1	18.5
Status at 15-Month Interview Date (%)						
Active in Phase 1	2.4	0.0	0.0	7.7	6.1	2.8
Active in Phase 2	9.8	4.3	14.0	7.7	36.7	15.5
Inactive	2.4	2.1	0.0	0.0	0.0	0.9
Terminated/ transitioned	85.4	93.6	86.0	84.6	57.1	80.8
Percent with Paid Employment in Phase 1	95.5	82.0	100.0	88.9	91.8	92.0
Percent with Paid Employment in Phase 2	65.9	62.0	80.4	40.7	99.4	66.4
Number in Sample:						
6-month interview	29	30	36	18	33	146
15-month interview	41	47	50	26	49	213
Any follow-up interview	44	50	56	27	49	226

NOTE: Data on paid employment in Phase 1 and Phase 2 were obtained from the Monthly Activity Forms in the STETS Information System; data on status at interview date were obtained from the Monthly Status Change Forms in the STETS Information System.

TABLE III.5

PLACEMENT AND FOLLOW-UP OUTCOMES OF EXPERIMENTAL GROUP MEMBERS

	Site					
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Status at Termination (%)						
Employed in regular job	38.6	32.0	51.8	29.6	59.2	43.8
Males	45.2	41.9	63.3	35.7	66.7	51.4
Females	23.1	15.8	38.5	23.1	52.0	33.3
Employed in workshop or in school or training	6.8	14.0	3.6	14.8	14.3	10.2
Males	3.2	16.1	6.7	7.1	16.7	10.0
Females	15.4	10.5	0.0	23.1	12.0	10.4
Other	54.5	54.0	44.6	55.6	26.5	46.0
Males	51.6	41.9	30.0	57.1	16.7	38.5
Females	61.5	73.7	61.5	53.8	36.0	56.2
Nature of Regular Jobs Held by Those Terminated to Unsubsidized Employment	11.8	18.7	3.6	12.5	10.3	10.2
Sector of Employment (%)						
For-profit	88.2	50.0	82.1	87.5	86.2	79.6
Nonprofit	11.8	18.7	3.6	12.5	10.3	10.2
Public-sector	0.0	31.2	14.3	0.0	3.4	10.2
Percent rollover from Phase 2	88.2	31.3	39.3	100.0	79.3	63.3
Average hours per week	29.9	32.2	31.5	31.5	29.9	30.9
Average hourly wage rate	$3.39	$4.35	$3.89	$3.84	$3.33	$3.71
Number in Sample						
Total[a]	44	50	56	27	49	226
Employed in unsubsidized job	17	16	28	8	29	98

NOTE: Data on final status and placements were obtained from the Monthly Status Change Forms in the STETS Information System.

[a] One sample member who transitioned into unsubsidized employment had missing data on the characteristics of the job.

10 percent of both the males and the females transitioned into a sheltered workshop or an educational or vocational training program, while 46 percent were terminated from STETS without having been placed in another position by STETS counselors. The most common reason cited by projects for nonpositive terminations was unsatisfactory attendance (comprising 18 percent of the cases), followed by an inability to perform job tasks, to manage personal health problems, and to manage other family/personal problems, each of which accounted for about 10 percent of the terminations. These impediments to regular job-holding tended to manifest themselves early in the program; 57 percent of the nonpositive terminations occurred before participants had any Phase 2 paid employment.

As shown in Table III.5, 63 percent of the jobs for those whose final status was transition into an unsubsidized job were rollovers from Phase 2 jobs.[1] Most of the placement jobs were with for-profit companies; Los Angeles was the only site that placed sizeable proportions of its participants (31 percent) in public-sector jobs. The jobs were characterized by an average of 31 hours of work per week, at an average starting wage of $3.71 per hour, translating into a starting pay level of $115 per week. Although the sites varied somewhat in terms of these aspects of the unsubsidized jobs held by former participants, the differences between males and females were minimal.

STETS participants continued to be contacted by telephone and in person by STETS counselors after having been placed in unsubsidized employment as part of Phase 3. However, no specific data on the level or frequency of such maintenance contacts were collected.

5. The Validity of the Control Methodology

We noted earlier that random assignment was critical to the research design because it was to generate a control group whose behavior would be similar to the behavior of the experimental group had the latter not participated in STETS. Thus, experimental effects could be estimated directly from the differences in behavior between the two groups at specific time periods. Ideally, prior to program intervention, the average characteristics of the two groups would be identical, so as to enable us to base the analysis largely on simple comparisons between the groups. However, with random assignment, some probability always exists that the samples will differ along some dimensions by chance; the probability of such differences occurring is inversely related to sample size. Although small differences between the groups can be corrected by using reasonably sophisticated analytic procedures, major differences can undermine the analysis.

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[1] "Rollover jobs" are regular jobs in which the employee began as a trainee in the STETS program. The job was said to "roll over" from training to regular status when all STETS subsidies were ended and when the level of trainer contact was reduced to no more than a maintenance level.

As indicated by the data on the baseline demographic and personal characteristics of the control and experimental groups (which were presented in Table III.1), some differences in characteristics do exist. However, these differences usually involve only a few individuals. In terms of demographic characteristics, the only noteworthy difference is age. On average, the experimental group was distinctly older than the control group. In terms of personal characteristics, noteworthy differences are found for secondary handicaps, living arrangement, and pre-baseline activities. The experimental group also seemed somewhat more disadvantaged than the control group at baseline: secondary handicaps affected 39 percent of the former and only 32 percent of the latter. This finding is reinforced by the slightly higher percentage of experimentals with an organic cause of retardation, as indicated in referral agency records. Experimental group members exhibited greater living independence than did controls: they were less likely to have resided with parents, and almost twice as likely to have lived semi-independently or independently (15 versus 8 percent). However, since the vast majority of both groups lived with their parents, the apparent difference, although large in percentage terms, involves relatively few individuals. Finally, control group members were much more likely than experimental group members to have been attending school when referred to STETS, although no differences in labor-market-related activities between the groups are notable.

The differences that emerged from the randomization process do not seem to be extreme and are generally consistent with a valid application of a control group methodology to a modest-size sample. The differences that exist will be accounted for in the analysis so as not to bias the estimation of experimental effects. In part, this process will require using the analytic methodologies described in Section III.D.

C. DATA COLLECTION OUTCOMES

Our primary concern in the data collection plan was our ability to obtain complete and accurate interview data from primary respondents (i.e., sample members) or their proxy respondents. In fact, the interview response rate was very high, and the data appear to be complete and of high quality. Here, we briefly review the outcome of our data collection plan; we present more details on the results in Appendix B.

1. Interview Response Rates

We attempted to administer a baseline interview to all 467 individuals who were enrolled in the research sample; we attempted to conduct 15- and 22-month follow-up interviews with all sample members who completed the baseline interview, and we attempted to conduct a 6-month follow-up survey with only a randomly assigned two-thirds of the sample.[1] Table III.6 provides the results of the baseline and follow-up surveys with the primary sample members.

At each wave, the vast majority of the sample completed the assigned interviews, from 97 percent at baseline to 89 percent at the 22-month follow-up. Thus, for example, 86 percent of the original sample

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[1] We determined that a two-thirds sample was sufficiently large to detect program impacts on the activities of sample members at month 6, because the dominance of STETS activities among experimentals at that point would have generated large impacts among key employment-related outcomes. (For a given level of statistical precision, larger impacts require smaller sample sizes.) This decision to confine the 6-month survey to two-thirds of the sample conserved research funds for the crucial 15- and 22-month surveys.

TABLE III.6

RESULTS OF THE BASELINE AND FOLLOW-UP SURVEYS WITH THE PRIMARY SAMPLE MEMBERS

		Percent by Final Status		
	Number Assigned	Complete	Refusal[a]	Other Noncomplete[b]
Baseline Survey				
Experimentals	236	99.1	0.0	0.9
Controls	231	95.7	2.2	2.2
Total	467	97.4	1.1	1.5
6-Month Survey				
Experimentals	155	94.2	1.9	3.9
Controls	148	95.9	1.4	2.7
Total	303	95.0	1.6	3.4
15-Month Survey				
Experimentals	234	91.0	3.0	6.0
Controls	221	91.4	2.7	5.9
Total	455	91.2	2.9	5.9
22-Month Survey				
Experimentals	234	89.3	3.0	7.7
Controls	221	87.8	2.7	9.5
Total	455	88.6	2.9	8.6

[a] These are refusals either by the primary sample members themselves or by their parents or guardians.

[b] These include those sample members who could not be located, who were deceased or incarcerated, or who had moved beyond a 50-mile radius of the study site.

completed both the baseline interview and the 22-month follow-up (8.9 percent of 97 percent). In general, the completion rates of the experimental and control groups were quite similar, with the differences ranging from a 3 percentage point difference at baseline to a 2 percentage point difference at 22 months. Refusals accounted only for less than 3 percent of the interview assignments. Although control group members or their parents were more likely to have refused to be interviewed at baseline, probably because of their disappointment at being excluded from the STETS program, a slightly higher percentage of the experimental group refused to be interviewed in each of the follow-up waves. These overall results--high completion rates, low refusal rates, and small differences between the experimental and control groups--indicate the success of interviewing the primary sample.

Refusals. Despite our success, interview nonresponse can pose serious problems for the analysis. Sample members may have refused to cooperate with the survey for a number of reasons, some of which might be correlated with the treatment effects. For example, experimental group members who had unfavorable program experiences may have wished to sever any further contact with the demonstration, including the survey. Thus, even though completion rates were very high, we investigated the likelihood of nonresponse bias to determine whether it posed a threat to the analysis.

In an attempt to determine the sample member's employment status, we conducted a special mini-survey of individuals (or their caregivers) who refused to respond to the 22-month survey. From our contacts with 11 of the 13 refusals, we determined that 5 were employed and that 4 were unemployed; we could not determine the status of the final two. Further, each status (employed, unemployed, and status unknown) was equally prevalent among experimentals and controls. On this basis, we concluded that refusals were nearly random, and would not affect the analysis.[1]

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[1] Because each of the other reasons for interview noncompletion (e.g., moved out of area, unable to locate, deceased, etc.) accounted for very few sample members in each group, they would not likely cause problems.

Proxy Respondents. Another aspect of interview nonresponse pertains to using proxy respondents. We attempted to administer interviews to proxy respondents only when data on prespecified, critical items in the primary interview were missing or inconsistent. Based on a pilot survey conducted as part of the design activities in which we identified and interviewed proxy respondents for each sample member, we expected that approximately 30 percent of all sample member interviews would contain sufficient data problems to warrant administering an interview with a proxy. The actual survey outcomes, presented in Table III.7, show that, other than in the pilot survey (when proxy respondent interviews were always attempted), a proxy interview was administered less often than expected--in general, for less than 20 percent of the completed sample member interviews. In all but three instances, sample members identified a proxy and gave permission for an interview when one was deemed to be necessary, and the proxy interview was completed in virtually all cases. In addition to these high completion rates, proxy-interview response rates differ little between the experimental and control groups.

Not all of the data from the proxy respondent interviews were used to construct the data files for the analyses described in this report. The following rules were developed to determine when the proxy respondent would provide information for the primary respondents:

- The proxy data were substituted for the entire interview when the interviewers noted that the answers of the primary respondent were "very unreliable" or were "reliable on only some items," and that his or her speech was "completely or severely impaired," and when the interviewers identified a consistent problem with the interview that prompted them to decide to contact a proxy.
- The employment, training, and schooling module was replaced by proxy data if two or more flagged items were missing or if the primary respondent had been unable to answer one of the questions on basic activities (e.g., "Do you work or have a job now?").
- The entire transfer benefits module was replaced if the primary respondent reported receiving one or more types of cash benefits but no amount was given.

TABLE III.7

RESULTS OF THE BASELINE AND FOLLOW-UP SURVEYS WITH THE PROXY SAMPLE MEMBERS

	Percent of Primary Respondent Interviews Identified As Needing A Proxy Respondent	Percent of Attempted Proxy Respondent Interviewers Completed[a]
Baseline Survey		
Experimentals	50.9 (23.8)[b]	99.1
Controls	50.7 (26.7)[b]	99.1
Total	50.8 (25.3)[b]	99.1
6-Month Survey		
Experimentals	15.1	100.0
Controls	23.2	97.0
Total	19.1	98.2
15-Month Survey		
Experimentals	15.5	100.0
Controls	15.8	100.0
Total	15.7	100.0
22-Month Survey		
Experimentals	11.5	100.0
Controls	15.5	100.0
Total	13.4	100.0
Number in Sample:		
Baseline	455	229
Month 6	288	55
Month 15	415	65
Month 22	403	65

[a] These figures represent the percentage of the sample who were identified as needing a proxy interview and for whom consent was obtained for an interview with an identified proxy. At baseline, proxies were not identified or consent was not obtained for 3 sample members.

[b] The baseline pilot sample, in which a proxy-respondent interview was attempted for all sample members, is excluded from the numbers in parentheses; the total sample size, excluding the pilot sample, is 297.

- Individual critical items (such as occupation, hours worked, and earnings) were replaced when data were missing or inconsistent in the primary respondent interview.

As shown in Table III.8, only about 6 percent of the sample required proxy respondents to provide all interview items. Depending upon the interview wave, from 1 to 16 percent of the sample required proxy data on items pertaining to earnings or the amount of transfer payments. For these items, a substantial decline in the use of proxy data occurred in successive interview waves.

2. Data Quality

Because of the potential reporting problems associated with the primary respondents, we carefully checked the quality of the data whenever possible. In the initial stage of the baseline survey effort, we conducted a pilot study in an attempt to collect parallel data sets from sample members, proxy respondents, and agency records. This pilot study (Bloomenthal et al., 1982) confirmed the ability of most of the STETS sample to respond to research interviews and generally to provide complete and accurate data on themselves. Records and proxy respondents were not superior sources in terms of either completeness or data quality. Our decision was to rely on self-reports for most of the evaluation data, particularly for the follow-up data. As we noted, we developed explicit procedures to identify sample members who might tend to provide inaccurate or incomplete answers to interview questions, and to identify and interview proxy respondents in those cases.

Throughout the baseline and follow-up data collection process, we collected some data from program records and through service agency interviews that could be used to corroborate some key components of the interview data. A comparison of data from the main surveys and from these other sources revealed few problems with the survey data. For example, in comparing the survey data with the data collected from interviews administered to service agency staff, we found that, in all survey waves, fewer than 20 survey responses of any type were inaccurate. Thus, all

TABLE III.8

USE OF PROXY RESPONDENT DATA IN ANALYSIS
(Percent)

	Baseline	6-Month Follow-up	15-Month Follow-up	22-Month Follow-up
Proxy Data Used For:				
Entire Interview	5.9	6.6	5.5	5.7
Entire Employment, Training, and Schooling Module	0.4	0.0	0.0	0.5
Entire Transfer Benefit Module	15.6	9.0	7.7	6.7
Proxy Data Used For Individual Items:				
Occupation	0.2	0.0	0.0	0.0
Hours Worked	0.4	0.4	0.2	0.2
Earnings	4.6	3.5	2.7	1.2
Number in Sample	455	288	415	403

available evidence suggests that the survey data on which the analysis is based are of very high quality.

D. ANALYTIC METHODOLOGIES

As we described previously, this report focuses on three issues: whether STETS met its employment-related and other specified objectives, whether and which characteristics of its participants or of the local projects affected its success, and whether the effects of STETS were sufficiently large to justify its costs. The primary outcomes of interest and their associated hypotheses have also been described. We now discuss the analytic methodologies that address these outcomes and hypotheses.

1. Impact Analysis

As determined by the research design (i.e., the random assignment of individuals to experimental or control status), the experimental and control groups exhibited very similar pre-assignment characteristics; thus, STETS participation should generally be uncorrelated with these characteristics. However, we did observe a few differences among these pre-assignment characteristics. Therefore, calculating experimental-control differences simply in terms of the mean values of outcome variables may produce biased estimates of experimental effects for at least some outcome variables of interest.

Regression techniques are advantageous because they control statistically for sample differences of this type, and can be expected to produce unbiased estimates of experimental effects.[1] They also offer two other advantages over a simple comparison of mean values. First, regression analysis provides more powerful tests of the potential effects of STETS, because we can control statistically for the influences of other explanatory variables. Second, by including the explanatory variables in the regression model, we can directly assess their individual net influences on outcome variables within a simple analytical framework. Further, by interacting an experimental status variable with other explanatory variables, we can attempt to isolate the effects of STETS for various subgroups. Thus, through regression techniques, we can begin to address the questions of how, for whom, and under what conditions STETS benefits its participants.

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[1] Because actual participation in STETS was nearly universal among members of the experimental group, the analysis is not diluted by the inclusion of a high percentage of nonparticipants in the experimental group. Further, the relatively small number of nonparticipants (8 percent) does not warrant special techniques to isolate these individuals and their control-group counterparts.

What must underlie a regression analysis is a behavioral model that associates the outcomes of interest with a set of explanatory variables, some of which are predetermined or exogenous, but some of which may be simultaneously determined. However, a behavioral model cannot easily be defined for this study. For example, it is unclear how parents, guardians, advisors, and the mentally retarded sample members themselves interact to make decisions about such elements as job-holding, public assistance, or place of residence. If the sample members generally make their own decisions, it is uncertain how they perceive and react to traditional economic incentives. If other persons make or strongly influence the decisions, it is unclear who the appropriate persons are, or what their incentives may be. In either case, social and psychological factors may play a role that is potentially as great or greater than the role of economic factors.

Because of the uncertain nature of the behavioral relationships and the possible simultaneous determination of outcomes, the regression analysis is based on reduced-form equations that associate the outcome variables with a vector of demographic, personal, and program background characteristics that are all exogenous. These include all of the variables that define possible subgroups of interest, other background variables, and, of course, an experimental status variable. Relying on reduced-form models provides accurate estimates of net treatment effects, but does not allow these effects to be disaggregated into their direct and indirect components. For example, STETS may influence living arrangements directly by improving participants' self-confidence and their ability to function independently, but it may also have an indirect influence through its effect on postprogram earnings. In this analysis, the coefficient that is estimated for the experimental status variable in a living-arrangement equation will reflect the net effect of these direct and indirect influences.

These models have also been expanded by the inclusion of a series of experimental status variables interacted with variables denoting personal characteristics. These expanded models enable us to estimate the importance of key subgroup characteristics in determining the impacts of the program and to test the statistical significance of these estimates. For example, we can estimate the impacts of STETS for males and for females by assuming either that these two groups are similar in all other respects (the option we have chosen) or, alternatively, that they resemble the STETS samples of males and females in all respects. Our rationale for opting to report the ceteris paribus estimates for sample subgroups is that a comparison of the estimates for subgroups that are thus defined provides evidence on the overall importance of the attribute in determining the impacts of the program. However, as noted in our discussion on the sample design, the statistical power of these subgroup results is low, and the power of differential impact estimates is even lower. Therefore, our discussion of subgroup results focuses on patterns rather than on statistical significance.

The bulk of our analysis relies on ordinary least squares (OLS) regressions, with the various outcome variables analyzed separately for the 6-, 15-, and 22-month data. For key binary outcome variables, we verified the robustness of the estimated experimental effects by using more appropriate probit maximum likelihood equations, as are presented in Appendix A. However, the OLS estimates are featured for simplicity and consistency.

By necessity, we occasionally estimated experimental-control differences based on small subgroups of the research sample defined by one of the outcome measures. In such cases, the analysis is necessarily imprecise (as we will discuss in the next section), and the advantage of regression analysis is minimal. Hence, we calculated these experimental-control differences on the basis of simple differences of mean values, and we caution the reader to view these results as descriptive data to facilitate interpreting the overall impact results, rather than as, themselves, being suggestive of the program impacts.

2. Benefit-Cost Analysis

A key component of the evaluation is a benefit-cost analysis that provides a structured method for assessing whether the impacts generated by the program are sufficiently large to justify the costs. The analysis focuses on several estimates of net present value per participant--that is, the difference between average benefits and average costs. The term present value means that the values of benefits and costs that accrue in the future have been adjusted to reflect their value in the present. Benefits and costs are computed on a per-participant basis to control partially for program size and to facilitate drawing cross-program comparisons.

If all the benefits and costs were measured, the hypothesis test would be whether the net present value per participant exceeded zero. A positive net present value would indicate that the program represented a good use of resources, while a negative value would suggest that the resources could have been used more productively elsewhere in the economy. Of course, all benefits and costs cannot be measured, and many of those that are measured are measured imperfectly. Consequently, it is necessary to look beyond simple benefit-cost estimates to examine the relative uncertainties associated with the various estimates and the probable magnitudes of unmeasured benefits and costs.

The procedure adopted here is to emphasize the general patterns that emerge from attempting to assign relative values to effects. The analysis does not focus on a single net present value estimate, but rather on a set of estimates based on plausible assumptions and estimates. This set includes both a benchmark estimate that incorporates the assumptions and estimates with which we feel most comfortable and several alternative estimates based on sensitivity tests, each illustrating the effect of changing one of the assumptions used in the benchmark calculations (keeping the others unchanged).

Despite these procedures, a number of uncertainties remain. In particular, a number of program effects could not be explicitly valued--for example, preferences for working, a desire to provide opportunities to disadvantaged persons, and personal assessments of the risks and gains of entering the labor market. The benefit-cost analysis uses a comprehensive accounting framework that includes all of these effects to enable us to assess qualitatively the degree to which intangible effects could alter conclusions based on those effects that are valued. In essence, the analysis provides an estimate of net cost--the value of measured costs less the value of measured benefits. If STETS is to be judged a desirable investment, the net worth of the intangibles must be sufficient to offset the net costs. In cases where measured benefits exceed measured costs, the program is usually considered desirable.[1] In other cases, policy judgments must be made about whether the intangible benefits are likely to be sufficiently valuable to generate an overall positive net present value. To help render this type of judgment, our benefit-cost analysis includes a component which summarizes the key impacts that may be correlated with the intangible benefits and costs.

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[1] Of course, intangible costs could be large enough to offset the measured positive net present value. Thus, intangibles must be considered even when measured net present value is positive.

IV. IMPACTS ON EMPLOYMENT, EARNINGS, AND OTHER LABOR-MARKET OUTCOMES

The overriding goal of the STETS demonstration was to identify effective interventions and targeting strategies for integrating mentally retarded young persons into the competitive labor market. Thus, a major objective of the impact evaluation is to measure the effectiveness of the program in increasing the employment levels of and improving the quality of the jobs held by program participants. With the experimental design adopted for the demonstration, the effectiveness of the program in these areas can be estimated reliably by comparing the employment activities and outcomes of the experimental (participant) group with those of the randomly selected control group.

In this chapter, we first provide an overview of the expected labor-market experiences of the STETS participants had they not enrolled in the program. This overview is especially useful in providing a clear understanding of the nature and extent of the employment problems facing both this target population as a whole and selected subsets of this population. In Section B, we present the estimated overall impacts of the program on employment-related outcomes, and discuss the implications of these impacts in terms of the characteristics of the jobs held by the STETS target population. Section C discusses differences in the estimated impacts across key subgroups of the target population as defined by site, demographic and personal characteristics, and program experiences. The final section summarizes the main employment-related findings from the demonstration and highlights the policy implications of these results.

A. LABOR-MARKET EXPERIENCES IN THE ABSENCE OF STETS

We can best illustrate the nature of the employment problems faced by the STETS target population by examining the employment experiences of the control group. As shown in Table IV.1 and Figures IV.1 and IV.2, the employment prospects of these young adults will improve somewhat over time. To illustrate, the proportion who were employed increased from 35 to 45 percent over the 22 months after baseline, and over 40 percent of those

TABLE IV.1

EMPLOYMENT ACTIVITIES OF CONTROL GROUP MEMBERS, BY SITE

	Site					
Activity and Time Period	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent with Regular Job[a]						
Baseline[b]	7.0	0.0	3.9	14.8	8.7	6.2
Month 6	6.9	7.4	18.9	0.0	19.4	12.0
Month 15	2.3	7.7	37.5	14.8	15.9	16.3
Month 22	0.0	10.5	37.0	18.5	22.7	18.6
Percent in Training Job[c]						
Baseline[b]	20.9	21.4	21.6	7.4	6.5	16.3
Month 6	20.7	7.4	24.3	44.4	9.7	19.7
Month 15	9.1	7.7	10.4	25.9	15.9	12.9
Month 22	2.6	7.9	0.0	7.4	18.2	7.2
Percent in Workshop/ Activity Center						
Baseline[b]	11.6	11.9	9.8	11.1	17.4	12.4
Month 6	3.4	22.2	13.5	11.1	19.4	14.1
Month 15	11.4	12.8	14.6	22.2	18.2	15.3
Month 22	12.8	13.2	15.2	48.1	15.9	19.1
Percent with Any Paid Job						
Baseline[b]	39.5	33.3	35.3	33.3	32.6	34.9
Month 6	31.0	37.0	56.8	55.6	48.4	45.8
Month 15	22.7	28.2	62.5	63.0	50.0	44.6
Month 22	15.4	31.6	52.2	74.1	56.8	44.8
Number In Sample:						
Baseline	43	42	51	27	46	209
Month 6	29	27	37	18	31	142
Month 15	44	39	48	27	44	202
Month 22	39	38	46	27	44	194

NOTE: These data are unadjusted subgroup means.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] In some cases, baseline interviews were administered several weeks after random assignment.

[c] Training jobs include work-study jobs.

FIGURE IV.1

EMPLOYMENT TRENDS AMONG CONTROLS

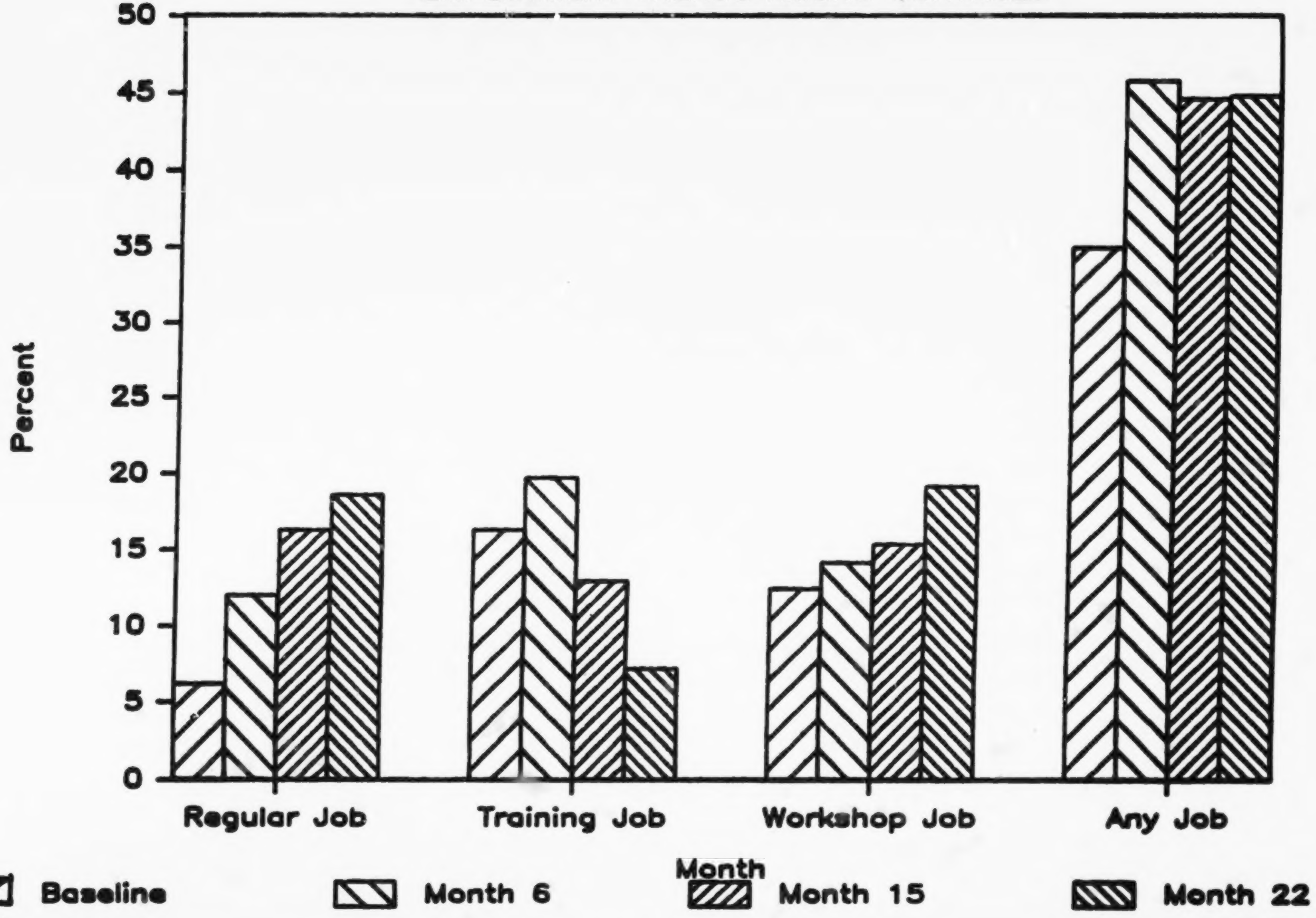
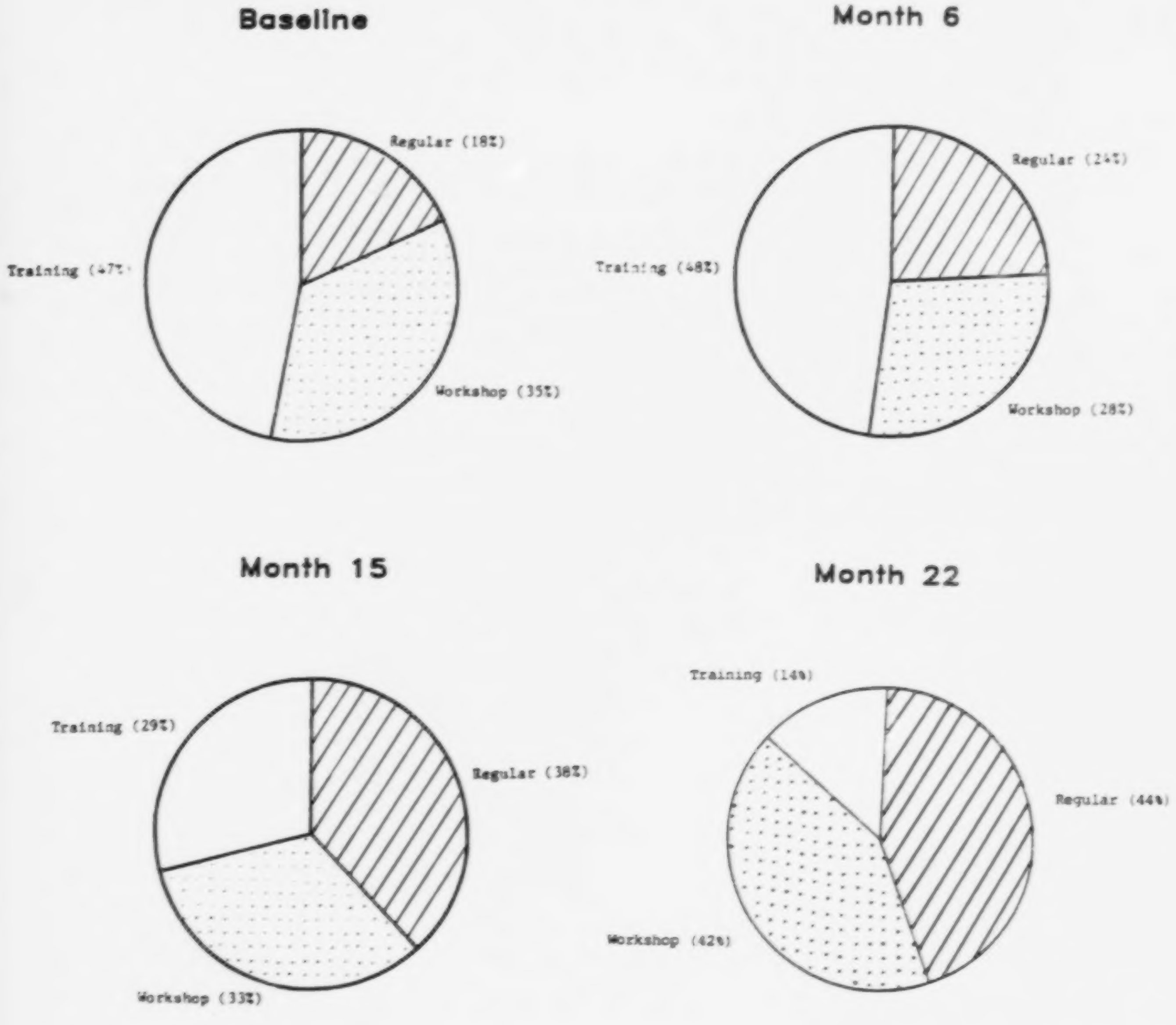


FIGURE IV.2

TRENDS IN TYPES OF JOBS HELD BY EMPLOYED CONTROL GROUP MEMBERS



NOTE: This figure is based on the regression-adjusted data presented in Table IV.4.

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who were employed in month 22 held regular jobs (nontraining jobs and non-workshop/activity-center jobs) that paid an average of $3.65 per hour (see Table IV.2.A).[1] Over half of these regular jobs were in the administrative-support and private-service fields (primarily messenger and food-service jobs), and most were full-time positions (more than 35 hours per week).

The increase in regular employment was due in part to the increase in the total number who were employed, and in part to the fact that some individuals moved from training jobs and workshop/activity centers into regular competitive employment jobs (see Figures IV.1 and IV.2)--both of which are trends that are associated with the aging of the target group. However, these employment levels and trends varied both across sites and among subgroups defined by other attributes--differences that will help both characterize the nature of the "successes" and highlight the nature and extent of the persistent problems.

Four especially notable differences in the employment experiences of individuals have been found across the five demonstration sites. First, there is evidence that control group members in Cincinnati lost ground under the economic conditions prevailing during the demonstration (see Table IV.1).[2] Second, controls in New York experienced by far the greatest gains in regular job employment (from 4 to 37 percent) and the greatest reductions in training-job employment over the 22-month observation period. Third, controls in St. Paul experienced the greatest overall gains in employment (from 33 to 74 percent), but over three-fourths of the jobs held by these individuals at the end of the demonstration period were workshop or activity-center jobs. Finally, the retention of regular jobs

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[1] Some portion of what we have termed "regular jobs" may have been subsidized through some federal, state, or local program. However, the overall incidence of subsidized jobs of this nature is sufficiently small that, for all intents and purposes, regular jobs as we have defined them can generally be thought of as competitive jobs.

[2] Appendix Table A.11 presents area unemployment rates in the demonstration sites throughout the time period covered by the evaluation.

TABLE IV.2

CHARACTERISTICS OF JOBS HELD BY CONTROL GROUP MEMBERS IN MONTH 22, BY SITE

A. REGULAR JOBS[a]

	Site					
Job Characteristics	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent of Sample with Weekly Earnings						
Less than $50	n.a.	100.0	11.8	20.0	0.0	19.4
$50 to $99	n.a.	0.0	11.8	40.0	50.0	25.0
$100 or more	n.a.	0.0	76.5	40.0	50.0	55.6
(Average weekly earnings)	(n.a.)	($32.51)	($132.54)	($90.07)	($101.39)	($106.87)
Percent of Sample with Hours Worked Per Week						
Less than 20	n.a.	25.0	23.5	20.0	20.0	22.2
20 to 34	n.a.	25.0	23.5	0.0	30.0	22.2
35 or more	n.a.	50.0	52.9	80.0	50.0	55.6
(Average hours per week)	(n.a.)	(30.2)	(30.2)	(34.6)	(34.8)	(32.1)
Percent of Sample with Hourly Wage Rate						
Less than $3.35	n.a.	100.0	41.2	40.0	70.0	55.6
$3.35 to $4.50	n.a.	0.0	5.9	60.0	30.0	19.4
$4.51 or higher	n.a.	0.0	52.9	0.0	0.0	25.0
(Average hourly wage rate)	(n.a.)	($1.36)	($4.77)	($2.82)	($3.10)	($3.65)
Percent of Sample with Occupation						
Sales	n.a.	0.0	11.8	0.0	20.0	11.1
Administrative, including clerical	n.a.	0.0	11.8	0.0	10.0	8.3
Administrative support[b]	n.a.	0.0	23.5	0.0	0.0	11.1
Private service[c]	n.a.	25.0	52.9	60.0	40.0	47.2
Fabricators, assemblers, and hand working	n.a.	0.0	0.0	0.0	20.0	5.6
Production inspectors	n.a.	0.0	0.0	0.0	0.0	0.0
Helpers, handlers, equipment cleaners	n.a.	0.0	0.0	0.0	0.0	0.0
Other	n.a.	75.0	0.0	40.0	10.0	16.7
Percent with Same Employer in Months 15 and 22[d]	n.a.	33.3	75.0	50.0	85.7	70.0
Percent with Regular Job in Months 15 and 22[d]	n.a.	33.3	87.5	75.0	85.7	80.0
Number in Regular Job	0	4	17	5	10	36
Percent in Regular Job	0.0	10.5	37.0	18.5	22.7	18.6

NOTE: These data are unadjusted subgroup means.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] This heading consists primarily of mail carrier and messenger jobs.

[c] This heading consists primarily of food service jobs.

[d] This sample includes all those who were employed in a regular job in month 15 and who completed a 22-month interview. The sample sizes are as follows: Cincinnati-0, Los Angeles-3, New York-16, St. Paul-4, Tucson-7, and total-30.

n.a. = not applicable

TABLE IV.2

CHARACTERISTICS OF JOBS HELD BY CONTROL GROUP MEMBERS IN MONTH 22, BY SITE

B. ALL PAID JOBS

	Site					
Job Characteristics	**Cincinnati**	**Los Angeles**	**New York**	**St. Paul**	**Tucson**	**Total**
Percent of Sample with Weekly Earnings						
Less than $50	100.0	83.3	33.3	52.6	43.5	52.4
$50 to $99	0.0	16.7	12.5	26.3	34.8	21.4
$100 or more	0.0	0.0	54.2	21.1	21.7	26.2
(Average weekly earnings)	($18.66)	($29.65)	($103.99)	($59.57)	($60.95)	($65.44)
Percent of Sample with Hours Worked Per Week						
Less than 20	16.7	25.0	20.8	10.5	17.4	17.9
20 to 34	66.7	41.7	41.7	21.1	52.2	41.7
35 or more	16.7	33.3	37.5	68.4	30.4	40.5
(Average hours per week)	(24.5)	(25.9)	(29.3)	(32.8)	(29.8)	(29.4)
Percent of Sample with Hourly Wage Rate						
Less than $0.75	50.0	50.0	8.3	26.3	30.4	27.4
$0.75 to $3.34	50.0	50.0	50.0	52.6	52.2	51.2
$3.35 or higher	0.0	0.0	41.7	21.1	17.4	21.4
(Average hourly wage rate)	($0.81)	($1.16)	($3.78)	($1.75)	($2.05)	($2.26)
Percent of Sample with Occupation						
Sales	0.0	0.0	8.3	0.0	13.0	6.0
Administrative, including clerical	0.0	0.0	12.5	0.0	4.3	4.8
Administrative support[a]	0.0	0.0	16.7	0.0	4.3	6.0
Private service[b]	33.3	16.7	45.8	26.3	21.7	29.8
Fabricators, assemblers, and hand working	50.0	33.8	12.5	47.4	17.4	27.4
Production inspectors	0.0	8.3	0.0	15.8	8.7	7.1
Helpers, handlers, equipment cleaners	16.7	8.3	4.2	0.0	13.0	7.1
Other	0.0	33.3	0.0	10.5	17.4	11.9
Percent with Same Employer in Months 15 and 22[c]	44.4	45.5	64.3	70.6	63.6	60.9
Percent with Job Months in 15 and 22[c]	55.6	45.5	75.0	88.2	95.5	77.0
Number in Paid Job	6	12	24	19	23	84
Percent in Paid Job	15.4	31.6	52.2	74.1	56.8	44.8

NOTE: These data are unadjusted subgroup means.

[a] This heading consists primarily of mail carrier and messenger jobs.

[b] This heading consists primarily of food service jobs.

[c] This sample includes all those who were employed in month 15 and who completed a 22-month interview. The sample sizes are as follows: Cincinnati-9, Los Angeles-11, New York-28, St. Paul-17, Tucson-22, and total-87.

over the follow-up period was substantially higher in New York and Tucson than in the other sites (see Table IV.2.A).

We also found some notable patterns in the incidence of job-holding and the types of jobs across different demographic subgroups. There appeared to be little variation in the probability of being employed, but substantial variation in the types of jobs held was evident. Table IV.3 shows that whites were much less likely than average to be without a job at month 22, as were those few persons who were living independently at baseline. Job-holding also varied considerably across sites; St. Paul had the lowest unemployment rate (26 percent) and Cincinnati had the highest (84 percent). However, the probability of being without a job did not vary substantially across the other subgroups.

In contrast, the types of jobs held varied considerably across all subgroups, with the exception of the male and female subgroups. In month 22, about 19 percent of the males and females held regular jobs; 7 percent held training jobs, and 20 percent held workshop or activity-center positions. In the absence of the intervention, the following sets of individuals would be more likely to have held regular jobs: individuals from the New York and Tucson sites, individuals who have only borderline retardation that does not have organic origins, individuals who are younger than age 22 at baseline, individuals who have some prior work experience, and individuals who have financial management skills. The following sets of individuals would be more likely to have held training jobs: individuals from the Tucson site, individuals who are moderately retarded, and individuals whose retardation has organic causes. Finally, the following sets of individuals would be more likely to have held a workshop/activity-center job: individuals from the St. Paul site, individuals who have mild or moderate retardation and/or retardation that has organic origins, and individuals who clearly exhibit dependency (as evidenced by their supervised living arrangement, their lack of money-management skills, and the presence of a benefactor). These patterns are consistent with prior expectations that the least handicapped individuals are those who were more likely to have moved into competitive employment in the absence of the intervention. Thus, the question to be addressed in the

TABLE IV.3

PERCENT OF CONTROL GROUP MEMBERS WITH VARIOUS EMPLOYMENT STATUSES IN MONTH 22, BY CHARACTERISTICS AT BASELINE

Subgroups Defined by Characteristics at Baseline	Employment Status			
	Not Employed	Regular Job[a]	Training Job[b]	Workshop/ Activity Center
Total Sample	55.2	18.6	7.2	19.1
Site				
Cincinnati	84.6	0.0	2.6	12.8
Los Angeles	68.4	10.5	7.9	13.2
New York	47.8	37.0	0.0	15.2
St. Paul	25.9	18.5	7.4	48.1
Tucson	43.2	22.7	18.2	15.9
IQ Level				
Borderline	53.3	31.7	6.7	8.3
Mild	58.3	14.8	3.5	23.5
Moderate	42.1	0.0	31.6	26.3
Age				
Younger than 22	58.9	20.6	7.8	12.8
22 or older	45.3	13.2	5.7	35.8
Gender				
Male	54.6	18.5	7.4	19.4
Female	55.8	18.6	7.0	18.6
Race/Ethnicity				
Black	71.9	14.1	3.1	10.9
Hispanic	62.1	24.1	3.4	10.3
White and other[c]	42.6	19.8	10.9	26.7
Living Arrangement				
Living with parents	56.1	18.9	7.9	17.1
Living in supervised setting	33.3	16.7	5.6	44.4
Living independently	72.7	18.2	0.0	9.1
Financial Management Skills				
Independent	59.6	29.8	2.1	8.5
Not independent	53.4	15.1	8.9	22.6
Receipt of Transfers				
SSI/SSDI	49.3	13.4	10.4	26.9
Other transfers only	63.2	17.6	8.8	10.3
No transfers	54.4	24.6	1.8	19.3
Secondary Handicaps				
Secondary handicap	53.2	17.7	12.9	16.1
No secondary handicap	56.1	18.9	4.5	20.5
Cause of Retardation				
Organic	46.7	6.7	16.7	30.0
Non-organic	56.7	20.7	5.5	17.1
Benefactor				
Benefactor	42.4	22.0	8.5	27.1
No benefactor	60.7	17.0	6.7	15.6
Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	51.9	25.9	14.8	7.4
Other job lasting ≥3 months	44.1	27.1	1.7	27.1
Other[d]	62.0	12.0	8.3	17.6
School Status at Referral				
Enrolled	60.3	12.3	12.3	15.1
Not enrolled	52.1	22.3	4.1	21.5
Number in Sample	107	36	14	37

NOTE: These data are unadjusted subgroup means. These figures sometimes differ slightly from regression-adjusted figures reported in other tables.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Training jobs include work-study jobs.

[c] Only 3 percent of the sample were classfied as "other",--American Indian or Asians.

[d] "Other" includes individuals with no prior work experience and those with less than three months of experience during the two years prior to their program application.

demonstration evaluation is whether these individuals or whether those who are less likely to have succeeded on their own constitute the group that would benefit most from the STETS treatment.

B. OVERALL PROGRAM EFFECTS

Most of the impact evaluation of the STETS program has been based on ordinary least squares (OLS) regression analysis. As noted in Chapter III, this technique is advantageous because it (1) controls for baseline differences between the experimental and control samples, (2) enables us to improve the efficiency of the program impact estimates over those that would have been generated on the basis of simple differences of means comparisons, and (3) facilitates investigating the impacts for a variety of sample subgroups. Moreover, OLS regression analysis is relatively inexpensive, which is an important consideration in view of the large number of outcome measures we are considering. However, OLS regression estimates of program impacts do not have desirable statistical properties when used with outcomes that are either truncated (i.e., which have lower and/or upper bounds) or binary; thus, we have re-estimated selected results by using appropriate maximum likelihood estimation techniques to confirm that, as is generally found in evaluations of this type, similar results will be obtained using OLS regression and maximum likelihood methods of estimation.[1]

The results of this analysis indicate that offering STETS services to mentally retarded young adults did have significant beneficial effects in terms of the incidence of competitive employment and the level of post-program earnings (see Table IV.4). Furthermore, these effects tended to remain fairly constant between months 15 and 22, despite the fact that some experimentals left their program jobs during this period.

In this section, we first discuss the estimated overall program impacts on employment in and earnings from various types of jobs for the

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[1] Appendix Table A.5 presents a comparison of selected regression and maximum likelihood estimates.

TABLE IV.4

ESTIMATED PROGRAM IMPACTS ON EMPLOYMENT ACTIVITY

	Month 6			Month 15			Month 22		
Outcome Measures	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Percent Employed									
Regular job[a]	11.8	10.7	1.1	26.2	16.8	9.4**	31.0	19.1	11.9**
Training job[b]	49.0	21.8	27.2**	14.0	12.9	1.1	6.7	6.4	0.3
Workshop/activity-center	6.8	13.1	-6.3*	4.9	14.6	-9.7**	7.0	18.3	-11.3**
Any paid job	67.8	45.2	22.6**	44.8	43.6	1.2	44.7	43.7	1.0
Average Hours Employed									
Regular job	3.9	3.0	0.9	7.8	5.0	2.8**	10.0	6.1	3.9**
Any paid job	19.8	12.2	7.6**	13.1	12.7	0.4	13.7	12.7	1.0
Average Weekly Earnings									
Regular job	$ 11.81	$ 9.81	$ 2.00	$ 26.90	$ 16.31	$ 10.59**	$ 36.36	$ 20.55	$ 15.81**
Any paid job[c]	52.39	25.93	26.46**	37.91	26.48	11.43**	40.79	28.41	12.38**
Number in Sample			283			402			395
Percent of Experimentals in Phase 1 and Phase 2	67.8			18.3			0.0		

NOTE: These results were estimated through ordinary least square techniques. Definitions and means of control variables that are included in the models are presented in Appendix Table A.1. Full results from a representative set of the impact equations that underlie these results are presented in Appendix Tables A.2 and A.3. Results for selected outcomes and time periods were also estimated using probit models that account for the binary nature of the outcome measures. These results are presented in Appendix Table A.5.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Training jobs include work-study jobs.

[c] In constant second quarter of 1982 dollars, these experimental-control differentials are slightly smaller. The differentials are $26.05, $10.50, and $10.10 in months 6, 15, and 22, respectively.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

full sample of experimentals.[1] We then discuss the effects of the program on the characteristics of the jobs held by those who are employed--effects that can be caused both by an induced change in the quality of jobs held by those who would have been employed in the absence of the program and by a program-induced movement of some individuals into employment.

1. In-Program Effects

Six months after program enrollment, when most (82 percent) of the experimentals were still enrolled in Phase 1 or 2 of the program and many (68 percent) were in program-funded training positions which paid minimum wages, experimentals were significantly more likely to be employed (68 versus 45 percent), to be working more hours per week on average (20 versus 12), and to be earning twice as much income per week ($52 versus $26). This increased employment among the STETS participants is attributable almost entirely to the program-provided training jobs.

This influence of the STETS training jobs can be seen quite clearly in Tables IV.5.A and IV.5.B, which show experimental-control group comparisons in terms of the characteristics of the jobs held. First, employed experimentals were much more likely than employed controls to be working in administrative, administrative support, and fabrication, assembling, and handworking jobs--occupations that were prevalent in the STETS Phase I and Phase II training jobs (see Riccio and Price, 1984, p. 43). Second, employed experimentals were more than twice as likely to be working full time (47 versus 22 percent). However, experimentals were 12 percent more likely to be earning less than the minimum wage (85 versus 76 percent).

2. Postprogram Effects

Under program guidelines, participants were to have spent no longer than 15 months in STETS. Thus, it was expected that both the 15- and 22-

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[1] Chow-tests of the acceptability of pooling data across sites indicated no evidence of different underlying structural models for the various sites. Selected results of these tests are presented in Appendix Table A.6.

TABLE IV.5

ESTIMATED PROGRAM IMPACTS ON JOB CHARACTERISTICS

A. REGULAR JOBS[a]

	Month 6			Month 15			Month 22		
Job Characteristics	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Average Weekly Earnings	$ 102.30	$ 89.70	$ 12.60	$ 103.44	$ 97.52	$ 5.92	$ 118.31	$ 106.87	$ 11.44
Average Hours Per Week	33.4	27.2	6.2*	29.5	30.0	-0.5	32.0	31.9	0.1
Average Hourly Wage	$ 3.10	$ 3.32	$ -0.22	$ 3.65	$ 3.47	$ 0.18	$ 3.62	$ 3.65	$ -0.03
Percent of Sample with Weekly Earnings			[e]						
Less than $50	12.5	25.0	-12.5	12.7	18.2	-5.5	11.1	19.4	-8.3
$50 to $99	12.5	37.5	-25.0	27.3	30.3	-3.0	25.4	25.0	0.4
$100 or more	75.0	37.5	37.5	60.0	51.5	8.5	63.5	55.6	7.9
Percent of Sample with Hours Worked Per Week			[e]			*			
Less than 20	12.5	23.5	-11.0	24.6	18.2	6.4	12.5	22.2	-9.7
20 to 34	25.0	41.2	-16.2	21.0	45.5	-24.5	28.1	22.2	5.9
35 or more	62.5	35.3	27.2	54.4	36.3	18.1	59.4	55.6	3.8
Percent of Sample with Hourly Wage Rate			[e]						
Less than $3.35	68.8	50.0	18.8	50.9	45.5	5.4	50.8	55.6	-4.8
$3.35 to $4.50	18.7	31.2	-12.5	30.9	27.3	3.6	30.2	19.4	10.8
$4.51 or higher	12.5	18.8	-6.3	18.2	24.2	-6.0	19.0	25.0	-6.0
Percent of Sample with Occupation			[e]						*
Sales	0.0	5.9	-5.9	1.8	0.0	1.8	1.6	11.1	-9.5
Administrative, including clerical	0.0	11.8	-11.8	7.0	6.1	0.9	9.4	8.3	1.1
Administrative support[b]	6.2	23.5	-17.3	12.3	15.2	-2.9	15.6	11.1	4.5
Private service[c]	50.0	35.3	14.7	52.6	51.5	1.1	45.3	47.2	-1.9
Fabricators, assemblers, and hand working	18.8	5.9	12.9	7.0	3.0	4.0	14.1	5.6	8.5
Production inspectors	0.0	5.9	-5.9	1.8	0.0	1.8	1.6	0.0	1.6
Helpers, handlers, equipment cleaners	12.5	5.9	6.6	14.0	18.2	-4.2	6.3	0.0	6.3
Other	12.5	5.9	6.6	3.5	6.0	-2.5	6.2	16.7	-10.5
Percent with Same Employer in Months 15 and 22[d]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	63.6	70.0	-6.4
Percent with Regular Job in Months 15 and 22[d]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	74.5	80.0	-5.5
Number in Regular Job[f]			33			90			100

NOTE: These results are based on unadjusted mean values and percentage distributions for those individuals who held regular jobs at the reference period. The tests of statistical significance are based on t-tests or chi-square tests, as appropriate. Some of the figures reported are based on slightly smaller sample sizes than indicated at the bottom of the table, due to missing data. These figures may differ slightly from the corresponding data reported in Table IV.2, due to slight difference in the sample. This sample is restricted to those cases with valid data on the main employment-related outcomes.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] This category consists primarily of mail carrier and messenger jobs.

[c] This category consists primarily of food service jobs.

[d] This sample includes all those who were employed in a regular job in month 15 and who completed a 22-month interview. The sample sizes are as follows: experimental group-55, control group-30, and total-85.

[e] Chi-square statistic could not be computed because over 20 percent of the cells have expected counts of less than 5.

[f] Because this table includes only those who held regular jobs, the proportions of experimental group and control group members are not approximately equal, as they are in other tables. At months 15 and 22, the sample consisted of approximately 63 percent experimentals and 37 percent controls, while the proportions at month 6 were nearly equal.

*Experimental-control differentials in the distributions/means are statistically significant at the 10 percent level, two-tailed test.
**Experimental-control differentials in the distributions/means are statistically significant at the 5 percent level, two-tailed test.

n.a. = not applicable

TABLE IV.5

ESTIMATED PROGRAM IMPACTS ON JOB CHARACTERISTICS

B. ALL PAID JOBS

Estimated Job Characteristics	Month 6 Experimental Group Mean	Month 6 Control Group Mean	Month 6 Estimated Impact	Month 15 Experimental Group Mean	Month 15 Control Group Mean	Month 15 Estimated Impact	Month 22 Experimental Group Mean	Month 22 Control Group Mean	Month 22 Estimated Impact
Average Weekly Earnings	$ 74.99	$ 56.11	$ 18.88**	$ 84.33	$ 58.30	$ 26.03**	$ 90.32	$ 64.18	$ 26.14**
Average Hours Per Week	29.2	26.9	2.3	29.3	29.0	0.3	30.6	29.2	1.4
Average Hourly Wage	$ 2.61	$ 2.25	$ 0.36	$ 2.96	$ .03	$ 0.93**	$ 2.85	$ 2.24	$ 0.61
Percent of Sample with Weekly Earnings			**			**			**
Less than $50	23.2	56.5	-33.3	29.5	52.9	-23.4	31.5	53.5	-22.0
$50 to $99	54.6	27.4	27.2	28.4	23.0	5.4	25.0	20.9	4.1
$100 or more	22.2	16.1	6.1	42.1	24.1	18.0	43.5	25.6	17.9
Percent of Sample with Hours Worked Per Week			**						
Less than 20	17.0	21.5	-4.5	18.3	18.9	-0.6	13.8	18.6	-4.8
20 to 34	36.0	56.9	-20.9	38.4	46.7	-8.3	39.4	40.7	-1.3
35 or more	47.0	21.5	25.5	43.3	34.4	8.9	46.8	40.7	6.1
Percent of Sample with Hourly Wage Rate			**			**			**
Less than $0.75	4.0	22.6	-18.6	10.5	32.2	-21.7	13.0	28.2	-15.2
$0.75 to $3.34	80.8	53.2	27.6	54.7	47.1	7.6	52.2	50.6	1.6
$3.35 or higher	15.2	24.2	-9.0	34.7	20.7	14.0	34.8	21.2	13.6
Percent of Sample with Occupation									**
Sales	0.0	1.5	-1.5	1.9	1.1	0.8	1.1	5.8	-4.7
Administrative, including clerical	7.1	4.6	2.5	3.9	4.5	-0.6	6.4	4.6	1.8
Administrative support[a]	12.1	9.2	2.9	8.7	8.0	0.7	11.8	5.8	6.0
Private service[b]	30.3	29.2	1.1	38.5	29.6	8.9	39.8	30.2	9.6
Fabricators, assemblers, and hand working	23.2	20.0	3.2	15.4	14.8	0.6	15.1	27.9	-12.8
Production inspectors	2.0	6.2	-4.2	1.9	4.5	-2.6	3.2	7.0	-3.8
Helpers, handlers, equipment cleaners	6.1	6.1	0.0	16.3	13.6	2.7	7.5	11.6	-4.1
Other	19.2	23.1	-3.9	13.5	23.9	-10.4	15.1	7.0	8.1
Percent with Same Employer in Months 15 and 22[c]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	54.0	60.9	-6.9
Percent with Regular Job in Months 15 and 22[c]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	72.0	77.0	-5.0
Number in Paid Job			163			194			180

NOTE: These results are based on unadjusted mean values and percentage distributions for those individuals who held jobs at the reference period. The tests of statistical significance are based on t-tests or chi-square tests, as appropriate. Some of the figures reported are based on slightly smaller sample sizes than indicated at the bottom of the table, due to missing data. These figures may differ slightly from the corresponding data reported in Table IV.2 due to slight differences in the sample. This sample is restricted to those cases with valid data on the main employment-related outcomes.

[a] This heading consists primarily of mail carrier and messenger jobs.

[b] This category consists primarily of food service jobs.

[c] This sample includes all those who were employed in month 15 and who completed a 22-month interview. The sample sizes are as follows: experimental group-100, control group-87, and total-187.

*Experimental-control differentials in the distributions/means are statistically significant at the 10 percent level, two-tailed test.
**Experimental-control differentials in the distributions/means are statistically significant at the 5 percent level, two-tailed test.

n.a. = not applicable

month data would have provided measures of postprogram impacts. In fact, however, 18 percent of the experimentals remained in STETS for 15 months or longer,[1] and thus the 15-month impact estimates reflect, in part, the direct influence of STETS employment and earnings. The 22-month results are truly postprogram impacts.

As shown in Table IV.4, the program clearly had its intended effects of increasing the likelihood of holding a competitive job, the average hours employed in regular jobs, and earnings. By month 15, 26 percent of the experimentals (compared with 17 percent of the controls) held regular jobs,[2] a 53 percent increase. A reduction in workshop/activity-center jobs of roughly equal magnitude (10 percentage points) was also evident in this time period, while no differences occurred in the proportions who held non-workshop-related training jobs.

Slightly larger but qualitatively similar results were obtained for month 22. The increases in the magnitudes of the estimated net impacts on regular and workshop job-holding were most likely due to the fact that many of the experimentals who were in training jobs in month 15 transitioned into these regular and workshop jobs.

Figure IV.3 shows quite clearly the shifts in the composition of the jobs held by experimentals relative to controls. As we noted above, in month 6, a substantially higher percentage of experimentals than controls had gained employment between the baseline and 6-month interviews, and, as shown in Figure IV.3, almost all of this gain was in program-related training jobs. By month 15, when the overall employment rates of experimentals and controls were similar, we began to see clear evidence that STETS greatly facilitated the movement of mentally retarded young adults out of workshop/activity-center training jobs into regular jobs, while

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[1] As noted in Chapter III, the average duration of enrollment was 10.6 months.

[2] Given that STETS jobs were considered training jobs, the fact that 18 percent of the experimentals were still in the program at this point causes a slight downward bias in this net impact estimate and an upward bias in the estimated impacts on training jobs.

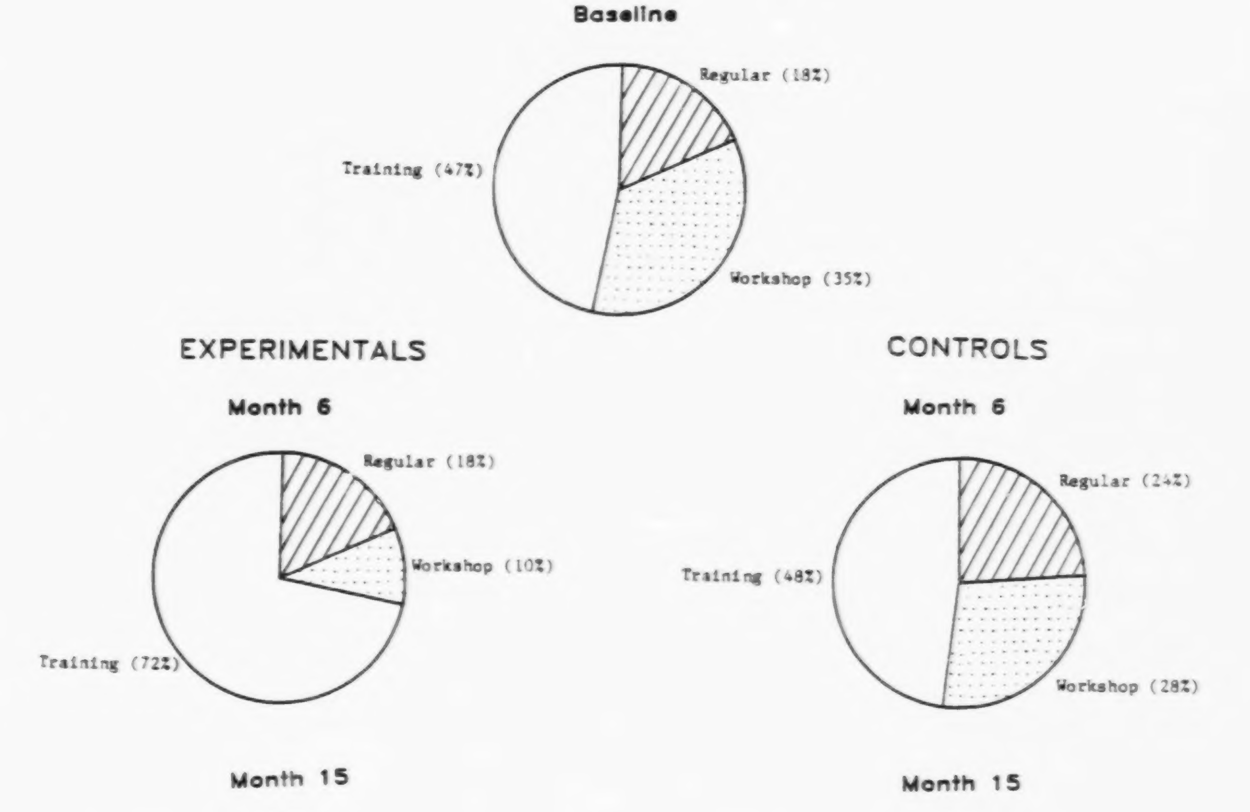

FIGURE IV.3

TRENDS IN TYPES OF JOBS HELD BY EMPLOYED EXPERIMENTALS AND CONTROLS

NOTE: This figure is based on the regression-adjusted data presented in Table IV.4.

control-group members exhibited a trend whereby they increased their overall level of employment and moved out of nonworkshop training jobs into both workshop/activity-center positions and competitive employment.

Accompanying these program-induced shifts from workshop/activity-center jobs to regular jobs was a significant gain by experimentals in average hours employed (from 5 to 8 hours per week in month 15, and from 6 to 10 hours per week in month 22). Average weekly earnings, both from regular and all paid jobs, also increased among experimentals, by between $11 and $16--an earnings differential that is proportionately larger than the employment-rate gains.[1]

Tables IV.5.A and IV.5.B present more detailed descriptive data on the quality of the jobs held by experimentals and controls. In the postprogram period (month 22), not only were experimentals more likely than controls to secure regular jobs, but the regular jobs also paid average hourly wages that were roughly equal to the regular jobs held by controls ($3.62 versus $3.65 per hour). When all jobs are considered, the average earnings gain of employed experimentals relative to employed controls was $26 per week, reflecting their higher incidence of holding regular jobs, which tended to pay substantially higher average hourly wage rates than paid in other types of jobs. Although a higher proportion of the employed experimentals held full-time jobs (47 versus 41 percent), this was not reflected in higher average hours of employment.

The final dimension in which the program appears to have induced a change in job characteristics pertains to occupation. In terms of general occupational categories, experimentals and controls held quite dissimilar jobs during the postprogram period. Roughly 40 percent of the employed experimentals worked in private-service positions (primarily food service), and sizeable numbers were employed in administrative-support positions (primarily mail carrier and messenger positions) and in fabrication and assembling/handworking positions (12 and 15 percent, respectively). In contrast, proportionately smaller numbers of controls were employed in private-service and administrative-support jobs (30 and 6 percent, respectively), and a correspondingly higher proportion were employed in fabrication and assembling/handworking positions. Most of these experimental-control differences are related to the differential incidence of regular job-holding, as is illustrated by comparing Table IV.5.A with Table IV.5.B.

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[1] In constant second-quarter 1982 dollars, these estimated effects on earnings from all jobs are $10.50 and $10.10, respectively--figures that are 92 and 82 percent of the nominal estimates for months 15 and 22, respectively.

It is interesting to note that both the experimentals and the controls had a fairly good record of job-holding. Among those who were employed in regular jobs in month 15, 75 to 80 percent were employed in month 22, and 64 to 70 percent were still with the same employer. The tenure rates for those in any job range from 54 to 77 percent. The slightly lower rates of job tenure among experimentals relative to controls are due primarily to the transition of some experimentals from STETS during this period.

C. PROGRAM EFFECTS BY SELECTED SUBGROUPS OF THE TARGET POPULATION

In an effort to better understand the mechanism through which STETS mitigates the employment problems of mentally retarded young adults and to help refine plans for targeting future interventions toward this population, we have examined estimates of program impacts for a large number of subgroups. In order to strengthen our understanding of the actual influence of a particular STETS project or of a participant's having a certain attribute on the impacts of the program, the net impact estimates presented for sample subgroups control statistically for differences among the subgroups in terms of factors other than those which define subgroup membership. For example, the net impact estimates reported for Cincinnati assume that the experimentals and controls in that site were similar to all sample members except for their city of residence; similarly, the results for males represent the difference in the expected value of the outcome measures for males in the experimental and control groups, assuming they have the sample mean values of all other characteristics. As was noted in Chapter III, because of both the limited sample size and the large number of subgroups of interest, the statistical power of this subgroup analysis is low. For this reason, we have tended to focus our discussion on patterns of results, especially in the postprogram period (months 15 and 22), and we caution the reader that these results should be viewed as tentative conclusions that warrant further substantiation based on future implementation and/or evaluation efforts.

As indicated by these results (which are reported in Tables IV.6.A and IV.6.B), estimated program impacts vary substantially across the five sites and among subgroups of the target population as defined by their personal and demographic characteristics. Furthermore, the patterns of these differential impacts tend to vary between the largely in-program period (month 6) and the largely postprogram periods (months 15 and 22) of observation.

1. Differences Across Sites

In month 6, sizeable differentials existed in the estimated net impacts on regular and any job-holding across sites (see Tables IV.6.A and IV.6.B)--differentials that can be traced largely to the variations in the nature and extent of program activities at that time. Of particular note is the relatively large (43 percentage point) impact on the likelihood of participants in Tucson holding any job in month 6--a result that is attributable in part to the higher proportion who were still in their STETS training jobs (see Table III.3 and Riccio, 1984, Table 3.2, p. 59). In contrast, St. Paul exhibited the largest impacts (31 percentage points) in regular job-holding, where experimentals were transitioned out of the program sooner than average, and where virtually none of the control group members held such jobs.

In months 15 and 22, when the results reflected primarily post-program impacts, St. Paul was the most effective site in moving target group members into competitive jobs (see Table IV.6.A).[1] In month 15, 29 percent of the experimentals in St. Paul were employed in such jobs, com-

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[1] Subgroup results for workshop participation, reported in Appendix Table A.13, show that the large positive increase in regular job-holding in St. Paul was offset by a larger reduction in workshop participation.

TABLE IV.6

ESTIMATED PROGRAM IMPACTS ON JOB HOLDING FOR KEY SUBGROUPS OF STETS PARTICIPANTS

A. PERCENT IN REGULAR JOB[a]

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	11.8	10.7	1.1	26.2	16.8	9.4**	31.0	19.1	11.9**
Site									
Cincinnati	7.0	6.5	0.5	19.2	5.9	13.3	17.9	1.6	16.3
Los Angeles	17.8	4.2	13.6	15.7	5.6	10.1	24.7	9.3	15.4
New York	3.5	13.7	-10.2	41.2	34.1	7.1	43.4	32.2	11.2
St. Paul	30.5	0.0[b]	30.5**	29.3	14.4	14.9	41.1	17.9	23.2*
Tucson	10.6	24.6	-14.0	24.6	20.5	4.1	29.6	30.8	-1.2
IQ Level									
Borderline	11.0	22.1	-11.1	24.9	28.7	-3.8	34.1	28.7	5.4
Mild	13.1	5.2	7.9	24.7	11.3	13.4**	28.1	16.0	12.1**
Moderate	7.7	9.4	-1.7	37.6	14.5	23.1*	38.2	10.7	27.5**
Age									
Younger than 22	11.0	10.6	0.4	27.7	21.5	6.2	30.2	22.2	8.0
22 or older	14.1	11.0	3.1	22.6	6.1	16.5**	32.5	12.2	20.3**
Gender									
Male	17.9	12.5	5.4	31.0	16.3	14.7**	35.5	18.2	17.3**
Female	3.9	8.4	-4.5	19.5	17.4	2.1	25.1	20.3	4.8
Race/Ethnicity									
Black	16.3	8.4	7.9	18.3	14.0	4.3	28.3	18.9	9.4
Hispanic	10.0	4.1	5.9	26.7	31.7	-5.0	48.4	25.8	22.6*
White and other	9.6	13.2	-3.6	30.6	14.4	16.2**	29.7	19.2	10.5*
Living Arrangement									
Living with parents	12.0	13.1	-1.1	26.1	15.4	10.7**	33.5	18.7	14.8**
Living in supervised setting	2.7	5.5	-2.8	16.3	25.0	-8.7	12.2	24.6	-12.4
Living independently	30.6	0.0[b]	30.6*	38.3	21.5	16.8	29.3	16.8	12.5
Financial Management Skills									
Independent	10.6	10.3	0.3	36.1	17.4	18.7**	41.9	23.9	18.0**
Not independent	12.4	10.9	1.5	22.3	16.5	5.8	26.9	17.3	9.6*
Receipt of Transfers									
SSI/SSDI	4.3	5.3	-1.0	20.4	12.7	7.7	27.7	14.5	13.2*
Other transfers only	13.4	12.1	1.3	34.6	16.5	18.1**	42.1	16.3	25.8**
No transfers	17.0	14.2	2.8	23.9	21.0	2.9	23.4	26.4	-3.0
Cause of Retardation									
Organic	13.8	13.1	0.7	15.2	13.4	1.8	33.6	9.9	23.7**
Non-organic	11.4	10.2	1.2	28.2	17.4	10.8**	30.4	21.0	9.4**
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	21.4	11.0	10.4	41.1	23.0	18.1	52.3	20.9	31.4**
Other job lasting >3 months	13.9	13.2	0.7	23.1	19.8	3.3	27.9	31.2	-3.3
Other	10.1	8.8	1.3	24.0	12.9	11.1**	26.9	10.5	16.4**
School Status at Referral									
Enrolled	12.0	6.0	6.0	20.1	8.9	11.2	27.0	14.0	13.0*
Not enrolled	11.7	12.7	-1.0	28.8	20.3	8.5*	31.9	21.4	10.5*
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table IV.4, these models include variables that interacted the treatment variable with the subgroup variables. The full regression results from which the 6- and 22-month results were derived are presented in Appendix Tables A.2 and A.3. Appendix Table A.12 presents the sample sizes for the various subgroups referenced in this table.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] The control group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

TABLE IV.6

ESTIMATED PROGRAM IMPACTS ON JOB HOLDING FOR KEY SUBGROUPS OF STETS PARTICIPANTS

B. PERCENT IN ANY PAID JOB

Subgroups Defined by Characteristics at Baseline	Month 6 Experimental Group Mean	Month 6 Control Group Mean	Month 6 Estimated Impact	Month 15 Experimental Group Mean	Month 15 Control Group Mean	Month 15 Estimated Impact	Month 22 Experimental Group Mean	Month 22 Control Group Mean	Month 22 Estimated Impact
Total Sample	67.8	45.2	22.6*	44.8	43.6	1.2	44.7	43.7	1.0
Site									
Cincinnati	50.9	33.8	17.1	31.5	22.9	8.6	28.3	13.4	14.9
Los Angeles	40.4	33.1	7.3	34.2	27.0	7.2	31.4	30.8	0.6
New York	78.3	54.8	23.5*	50.3	61.4	-11.1	52.0	54.0	-2.0
St. Paul	69.8	51.1	18.7	86.2	63.6	22.6	64.1	68.4	-4.3
Tucson	95.3	52.7	42.6**	66.9	48.6	18.3	52.9	57.8	-4.9
IQ Level									
Borderline	69.1	51.5	17.6	40.0	47.7	-7.7	44.1	44.4	-0.3
Mild	76.9	39.7	37.2	77.9	37.2	40.7	42.6	40.7	1.9
Moderate	62.5	56.1	6.4	62.4	68.3	-5.9	57.0	58.1	-1.1
Age									
Younger than 22	66.9	41.3	25.6**	44.6	41.1	3.5	41.6	41.6	0.0
22 or older	69.8	55.6	14.2	53.2	49.2	4.0	51.6	48.4	3.2
Gender									
Male	70.0	42.2	27.8**	55.1	45.6	9.5	52.2	46.3	5.9
Female	64.8	49.1	15.7*	30.8	40.9	-10.1	34.9	40.4	-5.5
Race/Ethnicity									
Black	65.3	39.2	26.1**	38.4	37.0	1.4	34.3	36.8	-2.5
Hispanic	84.5	35.0	49.5**	50.6	59.2	-8.6	63.9	38.0	25.9*
White and other	65.9	50.4	15.5*	47.0	43.3	3.7	45.6	49.1	-3.5
Living Arrangement									
Living with parents	66.1	49.0	17.1**	44.3	44.3	0.0	48.1	45.5	2.6
Living in supervised setting	80.3	46.3	34.0*	49.5	61.3	-11.8	-0.1	16.6	-16.7
Living independently	70.1	1.1	69.0**	44.1	16.2	27.9	58.6	52.1	6.5
Financial Management Skills									
Independent	66.0	46.8	19.2*	52.3	45.2	7.1	51.3	41.6	9.7
Not independent	68.5	44.5	24.0**	42.0	43.0	-1.0	42.2	44.5	-2.3
Receipt of Transfers									
SSI/SSDI	71.9	50.2	21.7*	42.6	45.3	-2.7	43.4	45.8	-2.4
Other transfers only	72.1	41.2	30.9**	51.3	39.5	11.8	54.3	41.2	13.1
No transfers	60.9	44.0	16.9*	40.8	45.7	-4.9	36.6	44.1	-7.5
Cause of Retardation									
Organic	62.6	46.8	15.8	44.1	36.5	7.6	54.4	41.0	13.4
Non-organic	68.8	44.9	23.9**	45.0	45.0	0.0	42.7	44.3	-1.6
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	78.3	39.7	38.6**	63.1	47.4	15.7	51.2	44.7	6.5
Other job lasting >3 months	64.4	50.7	13.7	44.5	55.1	-10.6	45.1	56.3	-11.2
Other	67.0	42.8	24.2**	39.7	34.6	5.1	42.4	35.0	7.4
School Status at Referral									
Enrolled	60.0	33.4	26.6**	38.1	46.7	-8.6	38.2	39.3	-1.1
Not Enrolled	71.1	50.3	20.8	47.8	42.2	5.6	47.6	45.7	1.9
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table IV.4, these models include variables that interacted the treatment variable with the subgroup variables. The full regression results from which the 6- and 22-month results were derived are presented in Appendix Tables A.2 and A.3. Appendix Table A.12 presents the sample sizes for the various subgroups referenced in this Table.

*Statistically significant at the 10 percent level, two-tailed test.

**Statistically significant at the 5 percent level, two-tailed test.

pared with only 14 percent of the controls. Cincinnati is the only site in which the program seemed to have increased overall employment by as late as month 22 (by an estimated 15 percentage points); experimentals in this site also increased their incidence of regular job-holding by a similar amount.[1] A notable observation in terms of this result is that overall employment among controls in Cincinnati was very low (13 percent), and almost none of this employment was in regular jobs. At the other extreme, the estimated post-program effects on regular job-holding in Tucson were relatively small (4 and -1 percentage points in months 15 and 22, respectively).[2]

2. Differences Among Other Sample Subgroups

Judging from the results reported in Table IV.6.A, it seems that STETS was most effective in increasing regular job-holding among four sample subgroups: those with mild or moderate retardation and whose retardation has organic causes, older individuals, males, and those who are more independent, as evidenced by their living arrangements and money-management skills--subgroups that tend to have average or lower-than-average probabilities of securing regular jobs on their own. Of these results, two sets are especially thought-provoking--the results on IQ levels and the results on gender. Estimated program impacts on the probability of holding a regular job are essentially zero for those participants with borderline IQ levels, 12 to 13 percentage points for those with mild retardation, and 23 to 28 percentage points for those whose IQ scores indicate moderate retardation. The net effect of these differential program results is that STETS tended to raise the employment prospects for the mild and moderate retardation subgroups, from levels well below those for the borderline retarded group to roughly similar levels. For example, in the absence of STETS, only 11 percent of the moderately retarded participants would have been expected to hold a regular job in month 22, compared with 29 percent of the borderline group; subsequent to STETS, 38 percent of the moderately retarded sample held such jobs, compared with 34 percent of the experimentals in the borderline group.

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[1] The cross-site comparisons of results for earnings outcome measures are qualitatively similar (see Appendix Table A.4).

[2] Large estimated impacts on any job-holding in month 15 are attributable largely to STETS training jobs.

One aspect of the estimated differential results for males and females is noteworthy--that STETS had large positive effects (14 and 17 percentage points in months 15 and 22, respectively) on males and essentially no effects on females, despite roughly equal probabilities that males and females in the control group would hold regular jobs in the postprogram period (16 to 20 percent). There is no clear explanation for this lack of program impacts for females. However, it is notable that the STETS programs, themselves, were less successful in serving women than men. As was discussed in Chapter III, a much higher proportion of the males who entered the program advanced to Phase 2 jobs (73 percent of the males versus 57 percent of the females), and the males tended to spend substantially more time in these jobs (as was shown in Table III.3). These findings are consistent with the reports by program operators that developing Phase 2 slots for females was somewhat more difficult than developing slots for males. However, this issue clearly warrants further investigation.

Table IV.6.B clearly shows that the overall null postprogram effects on the probability of holding any paid job also pertain to nearly all sample subgroups. The few hints that STETS may have increased overall levels of paid employment are associated with Hispanics, males, those with a higher-than-average degree of social independence, those whose retardation has organic causes, and those without significant recent workshop experience--all of which are groups that also exhibited higher-than-average increases in regular job-holding in month 22.

3. Differential Effects by Key Program Features and Characteristics

An important issue in assessing the STETS program model is the extent to which the nature or intensity of the program treatment affected the outcomes. This issue can also shed light on the male-female differences discussed above and may provide insight for monitoring and assessment purposes to identify short-term indicators of program performance. As is evident from the ongoing efforts at the federal level to refine employment-related program models and to define performance-monitoring criteria, this set of evaluation issues is difficult to address. Nonetheless, because of their importance, we have chosen to examine program effects that are associated both with three dimensions of program treatment (degree of program maturity at enrollment, program components entered, and hours of paid program-subsidized employment) and with one dimension of short-term performance (entered employment at termination).

For practical reasons, the demonstration design did not incorporate the random assignment of participants to various configurations of program services. Thus, the analysis becomes more complex and the results more tenuous, since elements of nonrandom self-selection and program assignment of individuals to various program treatments will undoubtedly have been present. In cases where nonrandom selection or assignment of individuals to program treatment occurs, it is likely that controlling for measured differences in personal characteristics will not fully account for the differentials in the expected performance of participants who receive various program services in the absence of the program treatment. Thus, if a typical impact regression model such as has been used to estimate the overall and subgroup analysis were used, the coefficients on the program treatment variables would measure the true effect of the particular program treatment, as well as the effects of unmeasured characteristics that affect both the self-selection or assignment to various treatments and the outcomes of interest.

The procedure we adopted for dealing with this problem in analytical terms was to use the instrumental variable procedure proposed by Maddala and Lee (1976).[1] In applying this procedure, we first estimated models of the probabilities that STETS participants entered the various program components, and that they entered employment upon leaving the program, and estimated a model to predict the length of time they spent in paid subsidized employment. We then used predicted measures of these program variables, rather than actual values, in the analysis. Because program maturity at enrollment was exogenous, estimating the impacts of the program on individuals who were enrolled at various times relative to the stage of program maturity did not necessitate using special estimation techniques.

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[1] Alternative procedures, such as those proposed by Heckman (1979) and Heckman and Robb (1983), could have been adopted with roughly equivalent results.

Table IV.7 summarizes the estimated overall program impacts for subgroups of experimentals defined by these three components of their STETS participation and the one component of the program's immediate outcome. These results suggest that an ongoing program would be expected to have greater impacts than did the STETS demonstration, which seemed to have had no beneficial impacts on those who enrolled during the start-up or phase-down periods of the programs. It is especially noteworthy that the programs tended to be quite effective for males, regardless of when they entered the program (see Table IV.8). Furthermore, the programs achieved substantial success with the females who enrolled during the "steady-state" period of program operations; the problem in achieving significant net impacts for females was concentrated in the periods when the programs were in transition (i.e., gearing up or phasing down), and it is these difficulties that account for the overall findings of no impacts during the early and later periods of program operations.

The results also indicate that relatively long periods of subsidized training jobs are important to the success of the STETS concept. Finally, the analysis provides some suggestion that "entered employment rates" can be used to gauge the success of programs. However, it is noteworthy that the sites which exhibited the largest impacts on employment in regular jobs were not always the same sites which exhibited the highest incidence of entering employment at the time individuals left the STETS program. For example, at one extreme, St. Paul exhibited the largest employment impacts and the lowest entered-employment rate; and, at the other extreme, Tucson exhibited no impacts on employment by month 22, but achieved the highest entered-employment rate. Thus, it seems obvious that

TABLE IV.7

ESTIMATED PROGRAM IMPACTS ON EMPLOYMENT ACTIVITY IN MONTH 22, BY PROGRAM EXPERIENCES

	Outcome Measure				
Subgroups Defined by Program Experiences	Percent of Experimentals In Subgroups	Percent in Regular Job[a]	Percent in Paid Job	Average Weekly Earnings Regular Jobs[a]	Average Weekly Earnings All Jobs
Stage of Program Operations At Enrollment[b]					
Early	21.6	0.4	-14.1	9.86	6.06
Steady state	60.3	15.4**	5.0	20.92**	15.17**
Late	18.1	11.1	3.2	4.19	2.09
Program Components Entered					
Phase I only	33.6	3.9	-2.9	2.58	-9.8
Phases I and II	66.4	16.3**	13.8*	21.76**	21.05**
Hours of Paid STETS Employment					
≤ 500 hours	30.9	-0.5	-22.4	18.50	-17.80
> 500 hours	69.1	17.3*	17.3	22.28**	22.21*
Termination					
Entered unsubsidized employment	43.8	28.5**	20.6**	36.92**	37.98**
Other	56.2	-0.1	-16.0*	0.71	-9.57
Number in Sample		403	403	402	400

NOTE: All of these estimates, with the exception of those pertaining to stage of program operations at the time of enrollment, were estimated using instrumental variables procedures such as have been proposed by Maddala and Lee (1976). The integrity of these results necessarily depends on the validity of untestable assumptions that underlie this approach for correcting for selection bias. For this reason, we offer these results more as exploratory findings that might provoke further examination within the context of future research and/or program efforts.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Program operations were divided into three periods: the early period in which the program was beginning operations, the "steady state" period in which program size and operations seemed reasonably stable, and the late period in which program enrollment was declining. The enrollment period corresponding to the steady state is a five-month period defined by site as follows: New York and Tucson (January to May 1982), Cincinnati (March to July 1982), St. Paul (April to August 1982), and Los Angeles (May-September 1982).

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

TABLE IV.8

ESTIMATED PROGRAM IMPACTS ON THE PERCENT HOLDING A REGULAR JOB AND ANY JOB IN MONTH 22, BY STAGE OF PROGRAM OPERATIONS AND GENDER

Stage of Program Operations at Enrollment[b]	Percent Holding a Regular Job			Percent Holding Any Paid Job		
	Males	Females	Total	Males	Females	Total
Early	15.6	-15.8	0.4	21.1	-16.7	-14.1
Steady State	18.9**	14.1*	15.4**	32.3**	1.3	5.0
Late	21.6	-10.2	11.1	20.1	-36.6*	3.2
Number in Sample	228	175	403	228	175	403

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Program operations were divided into three periods: the early period in which the program was beginning operations, the "steady state" period in which program size and operations seemed reasonably stable, and the late period in which program enrollment was declining. The enrollment period corresponding to the steady state is a five-month period defined by site as follows: New York and Tucson (January to May 1982), Cincinnati (March to July 1982), St. Paul (April to August 1982), and Los Angeles (May-September 1982).

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

entered-employment rates alone (i.e., without some adjustment for the characteristics of the client population served) should not be used as a key program performance measure.

Taken at face value, the results along each of these dimensions are quite strong. However, these findings should be considered only suggestive of the possible indicators of differential program effectiveness. Because of the inherent weaknesses in the analysis, noted above, they might be most valuable in terms of guiding future evaluation efforts.

D. SUMMARY AND CONCLUSIONS

This evaluation clearly indicates that a STETS-type program can be expected to improve the employment prospects of mentally retarded young adults. A STETS-type program will induce this effect primarily by helping participants transition out of workshop/activity centers into regular, competitive jobs. The results from this demonstration suggest that such a program can reduce the incidence of workshop jobs and increase the incidence of regular jobs by more than 60 percent (11 to 12 percentage points). These effects can be expected to be even larger in programs targeted toward subsets of the STETS target population, such as the mildly and moderately retarded and males. However, developing defensible targetting strategies that increase the proportions of the client population from these groups would be difficult, if not impossible. A more promising approach for improving program effectiveness would seem to be to provide a better understanding of and to mitigate the problems in serving females, and, relatedly, to increase the incidence of Phase 2 employment among all participants, but especially among females. Given the similarity of results for males and females during the "steady-state" period of program operations, it seems likely that this goal can be attained.

V. IMPACTS ON TRAINING AND SCHOOLING

An examination of the impacts of STETS on the likelihood that mentally retarded young adults will enroll in training or school over time is important for two reasons. First, STETS was largely an employment-training program, whose purpose was to move individuals out of training programs and schools into competitive employment. Thus, a complete assessment of the program should measure its success in reducing the long-term use of training and education programs. Second, since providing these types of services tends to be quite expensive, program-induced changes in participation in them can significantly affect the overall benefit-cost assessment of the program.

At the outset, we should note that the discussion in Chapter IV on the effects of the program on training and workshop job-holding and the discussion in this chapter on the effects of the program specifically on training overlap to a considerable extent. Overall, 83 to 93 percent of all training for control group members included some type of job component. In Chapter IV, we focused on regular job-holding, with training and workshop jobs constituting the residual categories of job-holding. In this chapter, we focus more directly on the impacts of the program on training itself.

We begin this chapter by providing an overview of the expected nature and level of training and schooling for STETS participants during the observation period had they not enrolled in the program. In Section B, we discuss the overall impacts of the program on the likelihood of enrolling in training or schooling. In Section C, we describe key differences in the impacts across sites and among sample subgroups defined by demographic and personal characteristics. The final section summarizes the key findings from this portion of the analysis and notes their main policy significance.

A. TRAINING AND SCHOOLING IN THE ABSENCE OF STETS

The behavior of the control group at baseline and at each of the follow-up waves (see Table V.1) indicates that 30 to 39 percent would have

TABLE V.1

SCHOOLING AND TRAINING OF CONTROL GROUP MEMBERS, BY SITE

Activity and Time Period	Site					
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent in Any Training						
Baseline[a]	16.7	40.5	44.9	14.8	27.9	30.5
Month 6	20.7	44.4	44.4	50.0	38.7	39.0
Month 15	25.0	28.2	29.2	46.2	27.3	29.9
Month 22	23.7	26.3	21.7	42.3	38.6	29.7
Percent in Training with a Job Component						
Baseline[a]	14.3	35.7	34.7	14.8	23.3	25.6
Month 6	20.7	40.7	41.7	50.0	32.3	36.2
Month 15	20.5	23.1	29.2	42.3	27.3	27.4
Month 22	21.1	21.1	19.6	42.3	25.6	24.6
Percent in Any School						
Baseline[a]	46.5	16.7	37.3	11.1	51.1	34.8
Month 6	17.2	29.6	21.6	11.1	12.9	19.0
Month 15	9.1	7.7	14.6	0.0	18.2	10.9
Month 22	7.7	18.4	8.7	3.7	20.5	12.4
Percent in School with Job Component						
Baseline[a]	23.3	2.4	25.5	0.0	27.7	17.6
Month 6	6.9	0.0	5.4	0.0	12.9	5.6
Month 15	4.5	0.0	0.0	0.0	13.6	4.0
Month 22	0.0	5.3	0.0	3.7	11.4	4.1
Number in Sample:						
Baseline	43	42	51	27	47	210
Month 6	29	27	37	18	31	142
Month 15	44	39	48	27	44	202
Month 22	39	38	46	27	44	194

NOTE: These data are unadjusted subgroup means.

[a] In some cases, baseline interviews were administered several weeks after random assignment.

been in training at any time, and that between 25 and 36 percent would have been in training that offered a job component. It is especially noteworthy that the incidence of training rose temporarily among controls, from 31 percent at the baseline period to 39 percent at month 6. This rise can probably be attributed to the fact that the agencies in St. Paul and Tucson referred many of the control group members to non-STETS programs, most of which included job components.

As shown in Table V.2, the characteristics of those who in the absence of STETS would have been more likely to enter a training program in the postprogram period parallel quite closely the characteristics of those who also would have been more likely to hold a paid job in this time period (see Table IV.3 in Chapter IV). They are mildly or moderately retarded, their retardation has organic causes, they are 22 years of age or older, they are white, they live in supervised settings, and they have not recently held a regular job for three or more months.

At the time of their referral to STETS, 35 percent of the control group were enrolled in schooling programs, half of which included a job component. As shown in Table V.1, the school enrollment rates dropped very quickly subsequent to baseline, so that by months 15 and 22 between 11 and 12 percent were enrolled in some type of school, and only 4 percent were enrolled in educational programs that offered a job component. By month 22, nearly two-thirds of those who were attending school were in either a regular or a vocational secondary school, and 78 percent of those schools offered special curricula for mentally retarded students (see Table V.3).

Notable differences across sites in terms of the incidence of school enrollment at referral parallel the differences across sites in terms of the proportion of referrals from the public school system (see Riccio and Price, 1984, Table 2.2, p. 33). However, only the school linkages in Los Angeles and Tucson show evidence of possible long-term influences on control group behavior; 18 percent of the controls in Los Angeles and 21 percent of those in Tucson were in school at the time of their 22-month interview (see Table V.2).

TABLE V.2

PERCENT OF CONTROL GROUP MEMBERS IN TRAINING AND SCHOOL IN MONTH 22, BY CHARACTERISTICS AT BASELINE

Subgroups Defined by Characteristics at Baseline	Individuals In Training	Individuals In School
Total Sample	29.7	12.5
Site		
Cincinnati	23.7	7.9
Los Angeles	26.3	18.4
New York	21.7	8.7
St. Paul	42.3	3.8
Tucson	38.6	20.5
IQ Level		
Borderline	18.3	11.7
Mild	33.6	11.5
Moderate	42.1	21.1
Age		
Younger than 22	23.6	16.4
22 or older	46.2	1.9
Gender		
Male	27.1	11.2
Female	32.9	14.1
Race/Ethnicity		
Black	18.8	7.8
Hispanic	20.7	6.9
White and other	39.4	17.2
Living Arrangement		
Living with parents	28.4	14.8
Living in supervised setting	50.0	0.0
Living independently	18.2	0.0
Financial Management Skills		
Independent	14.9	8.5
Not Independent	34.7	13.9
Receipt of Transfers		
SSI/SSDI	40.3	17.9
Other transfers only	23.5	8.8
No transfers	23.6	10.9
Secondary Handicaps		
Secondary handicap	34.4	13.1
No secondary handicap	27.5	12.2
Cause of Retardation		
Organic	56.7	16.7
Non-organic	24.7	11.7
Benefactor		
Benefactor	41.4	13.8
No benefactor	24.6	11.9
Work Experience in Two Years Prior to Enrollment		
Regular job lasting >3 months	22.2	3.7
Other job lasting >3 months	31.0	10.3
Other	30.8	15.9
School Status at Referral		
Enrolled	31.5	24.7
Not enrolled	28.6	5.0
Number in Sample	192	192

NOTE: These data are unadjusted subgroup means.

TABLE V.3

CHARACTERISTICS OF SCHOOLS ATTENDED BY CONTROL GROUP MEMBERS IN MONTH 22, BY SITE
(Percent Distribution)

School Characteristics	Site					Total
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	
Type of School						
Secondary school	66.7	14.3	0.0	0.0	62.5	37.8
Vocational school						
Secondary	0.0	0.0	100.0	100.0	12.5	26.1
Postsecondary	0.0	14.3	0.0	0.0	0.0	4.4
College[a]	0.0	14.3	0.0	0.0	25.0	13.0
Adult noncredit	33.3	57.1	0.0	0.0	0.0	21.7
Type of Curriculum						
School offers special curriculum for the mentally retarded	66.7	71.4	100.0	100.0	75.0	78.3
No special curriculum offered	33.3	28.6	0.0	0.0	25.0	21.7
Number in School	3	7	4	1	9	24

NOTE: Data on type of school were obtained from interviews with agencies that offered schooling (and other services). Agency representatives were asked to specify the primary services they provided, which may differ in some cases from the specific services that STETS sample members were receiving. The data are unadjusted subgroup means.

[a] These include community and junior colleges.

Those who were more likely to be in school during the postprogram period differ in two important dimensions from those who were more likely to be in training: they are younger than age 22, and they live with their parents. The age result simply highlights the fact that the vast majority of schooling-status changes involved leaving rather than enrolling in school. It also undoubtedly reflects the influence of P.L. 94-142, which mandates that schools provide free and appropriate education to mentally retarded individuals until they reach the age of 22 (see Chapter I).

B. OVERALL PROGRAM EFFECTS

In this section, we discuss the effects of the program on the incidence of training and schooling both during the in-program period and subsequent to program termination. As noted in Chapter IV, we have approximated these time periods by using the 6-month follow-up results as the basis for measuring the in-program effects, and the 15- and 22-month interview data as the basis for estimating the postprogram effects.

1. In-Program Effects

The influence of STETS training jobs is evident in the large positive increase in the incidence of training among experimentals relative to controls in month 6 (62 versus 41 percent), 86 percent of which arose from increases in training that offered a job component (see Table V.4). The impacts on school enrollment are negative, and all are substantially smaller than the training effects in absolute size. In month 6, only 8 percent of the experimentals, compared with 16 percent of the controls, were in school, and only 1 percent of the experimentals were in a school program that offered a job component. This finding undoubtedly reflects the fact that STETS directed the activities of individuals toward job training and away from other activity statuses (including school).

2. Postprogram Effects

The 15- and 22-month results reported in Table V.4 show that STETS reduced the incidence of both training and schooling during the postprogram

TABLE V.4

ESTIMATED PROGRAM IMPACTS ON TRAINING AND SCHOOLING

	Month 6			Month 15			Month 22		
Outcome Measures	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Training									
Percent in any training	61.7	40.6	21.1**	20.6	28.4	-7.8*	16.6	29.1	-12.5**
Percent in training with a job component	55.1	37.0	18.1**	18.2	26.7	-8.5**	13.5	24.9	-11.4**
Percent for whom training was the main activity	6.6	2.5	4.1*	2.1	2.6	-0.5	2.9	2.9	0.0
Schooling									
Percent in any school	7.5	15.7	-8.2**	6.2	10.1	-3.9	8.0	11.4	-3.4
Percent in school with job component	1.1	5.4	-4.3**	2.2	3.7	-1.5	2.0	4.6	-2.6*
Percent for whom school was the main activity	3.2	8.4	-5.2*	3.2	5.9	-2.7	6.3	6.4	-0.1
Number in Sample			283			402			395
Percent of Experimentals in Phase I and Phase II	67.8			18.3			0.0		

NOTE: These results were estimated through ordinary least square techniques. Definitions and means of control variables that are included in the models are presented in Appendix Table A.1. Full results from a representative set of the impact equations that underlie these results are presented in Appendix Tables A.7 and A.8.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

period. However, only the effects on training are consistently large and statistically significant.

In month 15, when 18 percent of the experimentals were still in STETS, 21 percent of the experimentals, compared with 28 percent of controls, were in training programs--a 7 percentage point differential. By month 22, the program-induced differential in the percentage who were in training had risen to 13 percentage points. In both time periods, these reductions were associated almost entirely with training that included a job component--primarily training in sheltered workshops and activity centers. Thus, the reductions have no overall effect on the likelihood that training rather than employment was the main activity of sample members.

C. PROGRAM EFFECTS BY SELECTED SUBGROUPS

In terms of the training and schooling results, the primary reason to examine program effects across sample subgroups is to develop a better understanding of the nature of the secondary consequences of the STETS intervention on the use of education and training resources. The a priori expectation was that the effects on such use would derive directly from the influence of the STETS treatment and from the program-induced changes in the incidence of competitive employment.

These expectations on the differential program effects were not fulfilled, as evidenced by comparing the results presented in Tables V.5.A and V.5.B with those that were presented in Tables IV.6.A and IV.6.B in Chapter IV. With a few exceptions, no noteworthy patterns of subgroup effects emerge for the training and schooling outcomes. Among the exceptions is the fact that the in-program effects on training are concentrated in three sites: Cincinnati (25 percentage points), New York (30 percentage points), and Tucson (43 percentage points). They also tend to be larger among subgroups which had lower probabilities of enrolling in training in the absence of the STETS demonstration: those who are younger than age 22, Hispanics, those who do not live in supervised settings, and those who are enrolled in school at the time of their referral.

TABLE V.5

ESTIMATED PROGRAM IMPACTS ON TRAINING AND SCHOOLING FOR KEY SUBGROUPS OF STETS PARTICIPANTS

A. PERCENT IN TRAINING

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	61.7	40.6	21.1**	20.6	28.4	-7.8*	16.6	29.1	-12.5**
Site									
Cincinnati	50.3	25.8	24.5	50.3	24.5	25.8	11.3	25.8	-14.5
Los Angeles	36.1	41.0	-4.9	26.0	27.6	-1.6	15.6	28.5	-12.9
New York	78.3	48.0	30.3**	13.1	29.0	-15.9*	14.8	28.4	-13.6*
St. Paul	42.5	45.2	-2.7	9.4	48.8	-39.4**	24.2	34.9	-10.7
Tucson	86.0	43.0	43.0**	32.4	20.6	11.8	22.1	30.0	-7.9
IQ Level									
Borderline	53.2	38.9	14.3	12.9	22.8	-9.9	14.6	22.0	-7.4
Mild	64.4	40.2	24.2**	22.8	28.7	-5.9	17.4	33.4	-16.**
Moderate	69.3	46.7	22.6	28.5	41.7	-13.2	21.9	24.3	-2.4
Age									
Younger than 22	62.2	34.9	27.3**	19.4	22.6	-3.2	15.2	22.5	-7.3
22 or older	60.4	56.2	4.2	23.1	41.3	-18.2**	19.7	43.6	-23.9**
Gender									
Male	56.9	40.2	16.7**	21.2	30.3	-9.1	18.4	28.9	-10.5*
Female	68.0	41.1	26.9**	19.6	25.7	-6.1	14.3	29.3	-15.0**
Race/Ethnicity									
Black	58.4	36.5	21.9*	18.6	27.1	-8.5	12.4	23.3	-10.9
Hispanic	69.8	31.9	37.9**	20.8	28.2	-7.4	12.0	16.6	-4.6
White and other	62.0	44.5	17.5**	21.4	29.1	-7.7	20.2	35.6	-15.4**
Living Arrangement									
Living with parents	61.2	40.1	21.1**	18.9	30.6	-11.7**	16.9	30.9	-14.0**
Living in supervised setting	79.0	65.6	13.4	50.2	39.1	11.1	26.0	31.5	-5.5
Living independently	45.1	14.0	31.1	8.9	0.0[a]	8.9	2.5	8.5	-6.0
Financial Management Skills									
Independent	67.6	44.7	22.9**	20.8	27.3	-6.5	11.6	22.4	-10.8
Not independent	59.3	38.9	20.4**	20.3	28.8	-8.5*	18.5	31.6	-13.1**
Receipt of Transfers									
SSI/SSDI	65.7	51.1	14.6	22.8	36.2	-13.4*	23.0	31.6	-8.6
Other transfers only	70.3	41.8	28.5**	18.3	27.8	-9.5	11.1	31.0	-19.9**
No transfers	51.9	30.9	21.0**	20.3	21.3	-1.0	15.6	24.7	-9.1
Cause of Retardation									
Organic	46.7	34.5	12.2	27.8	28.2	-0.4	22.8	48.4	-25.6**
Non-organic	64.6	41.8	22.8**	19.0	28.3	-9.3**	15.3	25.1	-9.8**
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	61.4	40.0	21.4	18.1	18.9	-0.8	5.9	27.7	-21.8*
Other job lasting >3 months	64.4	48.7	15.7	24.7	35.8	-11.1	19.3	26.5	-7.2
Other	59.8	34.8	25.0**	18.3	26.0	-7.7	17.9	31.2	-13.3**
School Status at Referral									
Enrolled	63.5	34.2	29.3**	19.9	34.0	-14.1*	13.7	29.2	-15.5*
Not enrolled	60.9	43.4	17.5**	20.8	25.8	-5.0	18.0	29.1	-11.1**
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table V.4, these models include variables that interacted the treatment variable with the subgroup variables. The full regression results that underlie the 6- and 22-month results are presented in Appendix Tables A.7 and A.8.

[a] The control group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

TABLE V.5

ESTIMATED PROGRAM IMPACTS ON TRAINING AND SCHOOLING FOR KEY SUBGROUPS OF STETS PARTICIPANTS

B. PERCENT IN SCHOOL

Subgroups Defined by Characteristics at Baseline	Month 6 Experimental Group Mean	Month 6 Control Group Mean	Month 6 Estimated Impact	Month 15 Experimental Group Mean	Month 15 Control Group Mean	Month 15 Estimated Impact	Month 22 Experimental Group Mean	Month 22 Control Group Mean	Month 22 Estimated Impact
Total Sample	7.5	15.7	-8.2**	6.2	10.1	-3.9	8.0	11.4	-3.4
Site									
Cincinnati	3.6	9.7	-6.1	5.1	9.5	-4.4	3.0	4.6	-1.6
Los Angeles	9.4	27.4	-18.0*	10.6	11.1	-0.5	15.8	22.8	-7.0
New York	8.5	12.8	-4.3	4.1	15.5	-11.4*	3.6	9.5	-5.9
St. Paul	7.8	17.6	-9.8	4.9	-3.0	7.9	3.3	2.8	0.5
Tucson	8.1	12.6	-4.5	6.2	11.3	-5.1	9.5	13.4	-3.9
IQ Level									
Borderline	12.7	11.4	1.3	6.3	15.1	-8.8*	5.2	9.6	-4.4
Mild	4.6	18.7	-14.1**	6.1	8.1	-2.0	10.5	10.2	0.3
Moderate	9.2	11.9	-2.7	6.9	7.5	-0.6	1.6	22.3	-20.7**
Age									
Younger than 22	7.5	15.3	-7.8	6.8	11.0	-4.2	8.9	15.5	-6.6*
22 or older	7.4	16.8	-9.4	4.8	8.0	-3.2	5.9	2.2	3.7
Gender									
Male	8.4	15.3	-6.9	4.2	7.9	-3.7	1.8	10.9	-9.1
Female	6.4	16.3	-9.9	9.0	13.1	-4.1	10.9	12.0	-1.1
Race/Ethnicity									
Black	7.7	16.0	-8.3	4.1b	6.9	-2.8	10.5	5.5	5.0
Hispanic	20.5	16.2	4.3	-0.0	14.8	-14.4*	4.1	0.4	3.7
White and other	4.9	15.4	-10.5*	4.9	14.3	-9.4**	7.6	17.5	-9.9**
Living Arrangement									
Living with parents	7.2	19.5a	-12.3**	7.7a	11.5	-3.8	9.2	13.8	-4.6*
Living in supervised setting	5.0	0.0	5.6	0.0	7.5	-7.5	-6.0	-1.6	-4.4
Living independently	22.9	3.0	19.9	0.0[a]	0.7	-0.5	10.7	3.5	7.2
Financial Management Skills									
Independent	5.2	26.5	-21.3**	9.6	11.5	-1.9	9.4	10.8	-1.4
Not independent	8.5	11.3	-2.8**	5.0	9.6	-4.6	7.5	11.6	-4.1
Receipt of Transfers									
SSI/SSDI	1.0	15.1	-14.1	4.3	20.5	-16.2	11.3	17.1	-5.8
Other transfers only	11.4	12.8	-1.4	5.5	1.9	3.6	7.3	8.6	-1.3
No transfers	10.0	18.4	-8.4	8.8	7.6	1.2	5.3	8.3	-3.0
Cause of Retardation									
Organic	8.5	23.0	-14.5	7.2	3.5	3.7	4.5	7.0	-2.5
Non-organic	7.3	14.3	-7.0	6.1	11.4	-5.3*	7.7	12.3	-4.6
Work Experience in Two Two Years Prior to Enrollment									
Regular job lasting >3 months	6.9	11.6	-4.7	1.3	7.6	-6.3	2.1	3.67	-1.6
Other job lasting >3 months	3.6	18.0	-14.4**	6.1	10.6	-4.5	10.0	10.7	-0.7
Other	10.6	15.3	-4.7	7.8	10.5	-2.7	8.3	14.0	-5.7
School Status at Referral									
Enrolled	7.2	24.1	-16.9**	5.6	17.6	-12.0*	7.4	10.9	-11.5*
Not enrolled	7.6	12.0	-4.4	6.5	6.7	-0.2	8.3	8.0	0.3
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table V.4, these models include variables that interacted the treatment variable with the subgroup variables. The full regression results that underlie the 6- and 22-month results are presented in Appendix Tables A.7 and A.8.

[a] The control group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

[b] The experimental group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

*Statistically significant at the 10 percent level, two-tailed test.

**Statistically significant at the 5 percent level, two-tailed test.

During the postprogram period, a few noteworthy patterns of subgroup results emerged. First, consistently larger reductions in training were observed among individuals who were 22 years of age or older, among individuals who were living with their parents, and among individuals who were enrolled in school at the time they were referred to STETS--subgroups that also tended to have experienced program-induced increases in their incidence of regular job-holding. Second, the program-induced reductions in school enrollment tended to be more consistently evidenced among whites and individuals who were enrolled in school at the time of their referral.

D. SUMMARY AND CONCLUSIONS

The combination of the STETS treatment itself and its effectiveness in increasing the incidence of competitive employment in the postprogram period had the expected effects of increasing the use of training programs (which offered job components) and of reducing the use of schooling during the in-program period and the use of both training and schooling during the postprogram period. Obviously associated with the reduction in the use of training and schooling programs is a social benefit in terms of lower resource expenditures (see the discussion on the benefit-cost analysis and its results in Chapter VIII). However, what we cannot answer from this demonstration is whether the STETS experience of those individuals who would have used more regular training and/or schooling had they not enrolled in STETS at least compensated for the foregone human capital from the non-STETS programs.

VI. IMPACTS ON PUBLIC TRANSFER DEPENDENCE

As illustrated by the impacts of STETS on the employment and training outcomes that were described in Chapter IV, the program did achieve some notable success in integrating mentally retarded young adults into the competitive labor market. These employment outcomes would be expected to have secondary effects on public transfer dependence, since the receipt and amount of most types of transfers are income-conditioned. In this chapter, we present the impacts of STETS on public transfers--both cash and in-kind transfers. Cash transfers received by sample members primarily include Supplemental Security Income (SSI), Social Security Disability Insurance (SSDI), Aid to Families with Dependent Children (AFDC), and general assistance. In-kind transfers include food stamps, Medicaid (in Tucson, the state equivalent), Medicare, and subsidized housing.[1]

We should first clarify several points about the process whereby we measured and defined transfers. First, survey respondents seemed to be able to report receiving SSI or SSDI, but were often unable to distinguish between the two. Because of the similarities between the two programs, we have simply combined the two. SSI recipients generally account for over 80 percent of the combined category of recipients. Second, we encountered the same problem for Medicaid and Medicare, which are usually (but not always) associated with, respectively, SSI and SSDI. Again, we have combined recipients of these two transfer sources into one category. Third, we encountered limited use of a variety of living arrangements. We constructed the category of subsidized housing to include living in group homes, supervised apartments, and institutions. (This concept is also considered in Chapter VII as a measure of independence.) Finally, several types of transfers--primarily welfare and food stamps--are likely to be associated with a combined family or household unit, rather than with the sample member only. We credited the sample member with having received such types of transfers only if the sample member (or a proxy respondent) reported that he or she was part of the administrative unit for each respective type.

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[1] These various income programs are described and their eligibility criteria are documented in The Social Security Bulletin, Annual Statistical Supplement. Washington, D.C.: U.S. Department of Health and Human Services, 1983.

We begin this chapter by providing an overview of the expected transfer use of STETS participants had they not enrolled in the program. This overview shows the receipt of each type of transfer both for the entire sample and for several important subsamples, and shows the amount of each type of cash transfer. Section B presents the estimated overall program impacts on transfer dependence, and Section C discusses differences in the estimated impacts across key subsamples. In the final section, we summarize the main transfer-related findings from the demonstration, and highlight the main policy implications of these findings.

A. PUBLIC TRANSFER DEPENDENCE IN THE ABSENCE OF STETS

On the whole, control group members exhibited very little change over time in either the receipt or the amount of public transfers, as shown in the last column of Table VI.1. This finding is generally counter to our expectations, since job-holding among controls increased by 28 percent over that period, and the proportion of the employed who held regular jobs increased from 18 percent at baseline to 42 percent at month 22 (see Table IV.1 in Chapter IV). Even with reporting and administrative lags, our hypotheses would tend to suggest a decline in transfer dependence.[1]

The trends are not necessarily consistent across types of transfers. Virtually no change occurred between baseline and months 15 and 22 in terms of the percentage who were receiving any cash transfers and the average monthly income from cash transfers.[2] However, both the percentage

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[1] One possible explanation for the observed results is an independent trend in increased SSI dependence as members of this target population aged.

[2] The trend often diverges for transfer use and benefit amounts measured at month 6, but this pattern is likely to be an artifact of the small sample size for month 6, rather than any real divergence from the longer-term trend.

TABLE VI.1

PUBLIC TRANSFER DEPENDENCE OF CONTROL GROUP MEMBERS, BY SITE

Type and Time Period	Site					Total
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	
Percent Receiving Any Cash Transfers						
Baseline[a]	41.9	51.2	56.9	44.4	69.6	53.8
Month 6	34.5	29.6	43.2	41.2	54.8	41.1
Month 15	55.8	46.2	40.4	48.1	65.9	51.5
Month 22	59.0	48.6	41.3	44.0	65.9	52.4
Average Monthly Income From Cash Transfers						
Baseline[a]	54.65	162.10	123.70	138.62	154.38	125.16
Month 6	57.00	110.70	76.17	98.24	133.03	94.40
Month 15	103.55	173.63	107.21	149.35	158.30	136.46
Month 22	110.74	195.68	103.82	79.39	163.43	133.44
Percent Receiving SSI or SSDI						
Baseline[a]	20.9	31.7	27.5	25.9	58.7	33.7
Month 6	20.7	25.9	24.3	11.8	54.8	29.1
Month 15	23.3	43.6	31.9	33.3	63.6	39.5
Month 22	28.2	43.2	32.6	20.0	63.6	39.3
Average Monthly Income From SSI or SSDI						
Baseline[a]	36.96	142.63	65.63	33.30	122.87	83.12
Month 6	41.93	105.70	43.24	24.89	133.03	72.13
Month 15	55.27	173.34	86.21	52.42	145.93	104.80
Month 22	74.10	183.97	87.07	42.11	152.71	111.83
Percent Receiving Other Cash Transfers[b]						
Baseline[a]	20.9	22.0	31.4	37.0	17.4	25.0
Month 6	17.2	3.7	24.3	29.4	0.0	14.2
Month 15	37.2	2.6	8.5	29.6	6.8	16.0
Month 22	30.8	5.4	8.7	28.0	6.8	14.7
Average Monthly Income From Other Cash Transfers[b]						
Baseline[a]	16.84	12.15	56.76	108.65	33.46	41.18
Month 6	13.57	5.00	31.72	71.88	0.00	20.71
Month 15	48.27	3.21	20.56	96.92	12.36	31.34
Month 22	36.64	11.40	16.39	35.65	10.73	20.80
Percent Receiving Food Stamps						
Baseline[a]	25.6	9.8	37.3	3.7	30.4	23.6
Month 6	24.1	0.0	24.3	11.8	25.8	18.4
Month 15	27.9	12.8	25.5	7.4	15.9	19.0
Month 22	23.1	10.8	32.6	16.0	18.2	20.9
Percent Using Medicare or Medicaid						
Baseline[a]	11.6	46.3	56.9	51.9	17.4	36.1
Month 6	6.9	44.4	56.8	41.2	22.6	34.8
Month 15	34.9	48.7	55.3	44.4	47.7	46.5
Month 22	38.5	45.9	50.0	60.0	52.3	48.7
Percent Living in Subsidized Housing						
Baseline[a]	11.6	9.8	2.0	37.0	10.9	12.0
Month 6	10.3	25.9	2.7	29.4	6.5	12.8
Month 15	9.3	15.4	4.3	29.6	13.6	13.0
Month 22	10.3	10.8	2.2	20.0	9.1	9.4
Number in Sample:						
Baseline	43	41	51	27	46	208
Month 6	29	27	37	17	31	141
Month 15	43	39	47	27	44	200
Month 22	39	37	46	25	44	191

NOTE: These data are unadjusted subgroup means.

[a] In some cases, baseline interviews were administered several weeks after random assignment.

[b] Other cash transfers primarily include AFDC and general assistance. However, some individuals received dependent and survivor Social Security benefits, special state or local stipends for training or housing, and Unemployment Insurance benefits.

who were receiving SSI or SSDI and the average monthly income from these two sources show small upward trends as the sample aged. The percentage change for SSI/SSDI income is twice as large as the percentage change for those who were receiving such income, indicating that, over time, more persons received SSI or SSDI, and that, on average, recipients received higher benefits from these programs. The opposite is true for other cash transfers.[1] Over time, both the percentage who were receiving other cash transfers and the average monthly income received by recipients fell. Virtually no change occurred over time in the percentage who were receiving food stamps or who were living in subsidized housing. The percentage who were using Medicare or Medicaid does show a trend of increased use.

A few site patterns are worth noting. First, with the possible exception of the percentage who were receiving SSI or SSDI in St. Paul, a modest upward trend occurred in both the receipt and amount of SSI or SSDI in all sites as the sample aged. Second, the percentage who were receiving other cash transfers declined in all sites except Cincinnati. In St. Paul, reductions also occurred in the average monthly income from these other transfers--reductions that are proportionately larger than the reductions in receipt, indicating sizeable reductions in the average monthly amount per recipient. In Tucson and New York, the proportional reductions in average monthly income roughly match the reductions in receipt, while, in Los Angeles, virtually no reductions in the average monthly benefit amount accompany the large reductions in receipt. Thus, recipients in Tucson and New York were basically receiving the same amount over time, while recipients in Los Angeles were receiving increased amounts over time. The situation was very different in Cincinnati: the percentage who were receiving other cash transfers increased over time (by 47 percent between baseline and month 22), and the average monthly income from those sources increased by a greater amount (by 118 percent between baseline and month 22). These trends for Cincinnati led to substantial increases in the benefit levels to recipients over time.

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[1] Although the composition changed over time, most of the "other cash benefits" are welfare--either AFDC or general assistance. The remaining portion is accounted for by a variety of programs, including dependent and survivor Social Security benefits, special state or local stipends for training or housing, and, especially at months 15 and 22, Unemployment Insurance benefits.

Just a few site differences are associated with the receipt of in-kind transfers. Although Los Angeles and New York show no real trends, both Tucson and Cincinnati show large increases in the use of Medicare or Medicaid over time. The trend in Tucson may be associated with the fact that Arizona actually did not have a Medicaid program per se, but instead operated a state substitute which underwent changes during the course of the demonstration. Enrolling in the revised program appears to have been easier than enrolling in the program that existed at the start of the demonstration. Modest trends occurred in St. Paul for all three types of in-kind transfers, the largest of which was the increase in the percentage who were receiving food stamps.

Table VI.2 shows the attributes of control group members that are associated with their receiving selected types of transfers in month 22. More seriously disadvantaged sample members--as indicated by lower IQ scores, the lack of financial management skills, the presence of secondary handicaps, and organic causes of retardation--were more likely to be receiving SSI or SSDI and Medicare or Medicaid, and less likely to be receiving other cash transfers. Control group members who were male, 22 years of age or older at baseline, or Hispanic or white were more likely to be receiving SSI or SSDI and Medicare or Medicaid than were others in the respective categories of subgroups. Sample members who were female, 22 years of age or older, or black were more like to be receiving other cash transfers. Baseline receipt of a certain type of transfer increased the likelihood of also receiving it at month 22. Finally, comparison group members who were living independently at baseline were much more likely to be receiving SSI or SSDI at month 22 than were those who were living in other arrangements, while those who were residing in supervised settings were more likely to be receiving other cash transfers and Medicare or Medicaid.

TABLE VI.2

PERCENT OF CONTROL GROUP MEMBERS RECEIVING TRANSFERS IN MONTH 22, BY CHARACTERISTICS AT BASELINE

Subgroups Defined by Characteristics at Baseline	Any Cash Transfers	SSI/SSDI	Other Cash Transfers[a]	Medicare/Medicaid
Total Sample	52.4	39.3	14.7	48.7
Site				
Cincinnati	59.0	28.2	30.8	38.5
Los Angeles	48.6	43.2	5.4	45.9
New York	41.3	32.6	8.7	50.0
St. Paul	44.0	20.0	28.0	60.0
Tucson	65.9	63.6	6.8	52.3
IQ Level				
Borderline	43.3	21.7	21.7	41.7
Mild	57.1	48.2	11.6	53.6
Moderate	52.6	42.1	10.5	42.1
Age				
Younger than 22	45.7	32.6	13.8	43.5
22 or older	69.8	56.6	17.0	62.3
Gender				
Male	53.8	41.5	12.3	50.0
Female	50.6	36.5	17.6	47.1
Race/Ethnicity				
Black	46.9	25.0	21.9	35.9
Hispanic	48.3	44.8	6.9	62.1
White and other	57.1	46.9	12.2	53.1
Living Arrangement				
Living with parents	47.9	35.6	13.5	46.6
Living in supervised setting	81.3	50.0	31.3	75.0
Living independently	72.7	72.7	9.1	45.5
Financial Management Skills				
Independent	45.7	28.3	19.6	37.0
Not independent	54.2	42.4	13.2	52.8
Receipt of Transfers				
SSI/SSDI	81.5	72.3	12.3	70.8
Other transfers only	48.5	26.5	23.5	48.5
No transfers	23.2	16.1	7.1	23.2
Secondary Handicaps				
Secondary handicap	68.9	59.0	13.1	65.6
No secondary handicap	44.6	30.0	15.4	40.8
Cause of Retardation				
Organic	82.8	82.8	3.4	55.2
Non-organic	46.9	31.5	16.7	47.5
Benefactor				
Benefactor	51.7	43.1	12.1	53.4
No benefactor	52.6	37.6	15.8	46.6
Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	37.0	22.2	18.5	33.3
Other job lasting >3 months	48.3	39.7	8.6	43.1
Other	58.5	43.4	17.0	55.7
School Status at Referral				
Enrolled	62.5	47.2	15.3	51.4
Not enrolled	46.2	34.5	14.3	47.1
Number in Sample	100	75	28	93

NOTE: These data are unadjusted subgroup means.

[a] Other cash transfers primarily include AFDC and general assistance. However, some individuals received dependent and survivor Social Security benefits, special state or local stipends for training or housing, and Unemployment Insurance benefits.

B. OVERALL PROGRAM EFFECTS

In this section, we discuss the overall impacts of the program measured at three points in time. We first discuss the impacts at month 6, which we have characterized as the in-program period. We then discuss the impacts at months 15 and 22, which we have characterized as the postprogram period.

1. In-Program Effects

As described at the start of this chapter, the impacts of the STETS program on employment that were described in Chapter IV led to the expectation that program participation would also reduce dependence on cash transfers. However, we also expected that the reduction would occur with a lag, since both reporting and administrative responses would not likely have kept up with changing labor-market activities. In fact, as shown in Table VI.3, the estimated experimental effects consistently show evidence of reduced dependence on all transfers other than food stamps. However, only the effects that were estimated for the percentage who were receiving any cash transfers, the percentage who were receiving cash transfers other than SSI and SSDI (primarily welfare), and the percentage who were living in subsidized housing are statistically significant. Experimentals were 26 percent less likely to be receiving any cash transfers (32 versus 43 percent), and were 58 percent less likely to be receiving cash transfers other than SSI and SSDI (6 versus 14 percent). A substantial effect also occurred in terms of the percentage who were living in subsidized housing--a reduction of about one-third (10 versus 15 percent). However, such an immediate effect on housing of this magnitude seems unusual, and this effect may well have been due to the small numbers of sample members who were living in subsidized housing (see Chapter VII, Section VII.B).

2. Postprogram Effects

The estimated experimental effects at month 15, by which time most experimentals were no longer in the program, followed the pattern established for month 6, but with some differences. The statistically significant effects estimated at month 6 for both the percentage who were

TABLE VI.3

ESTIMATED PROGRAM IMPACTS ON PUBLIC TRANSFER DEPENDENCE

	Month 6			Month 15			Month 22		
Outcome Measures	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Percent Receiving Any Cash Transfers	31.7	43.1	-11.4**	44.5	51.5	-7.0*	49.6	52.0	-2.4
Average Monthly Income from Cash Transfers	$80.23	$99.98	$-19.75	$114.78	$138.72	$-23.94	$126.53	$136.08	$-9.55
Percent Receiving SSI or SSDI	26.3	31.0	-4.7	33.1	40.7	-7.6**	34.9	40.2	-5.3
Average Monthly Income from SSI or SSDI	$66.41	$74.59	$-8.18	$91.35	$109.65	$-18.30	$99.27	$120.03	$-20.76
Percent Receiving Other Cash Transfers[a]	6.0	14.3	-8.3**	12.4	14.7	-2.3	18.0	13.4	4.6
Average Monthly Income from Other Transfers[a]	$13.04	$22.42	$-9.38	$22.26	$29.71	$-7.45	$29.23	$19.45	$9.78
Percent Receiving Food Stamps	22.0	18.4	3.6	22.1	18.0	4.1	21.8	19.8	2.0
Percent Using Medicare or Medicaid	29.7	35.9	-6.2	41.0	46.4	-5.4	45.9	48.1	-2.2
Percent Living in Subsidized Housing	9.9	15.3	-5.4*	13.7	13.9	-0.2	11.0	10.0	1.0
Number in Sample			287			413			398
Percent of Experimentals in Phase 1 and 2	67.8			18.3			0.0		

NOTE: These results were estimated through ordinary least squares techniques. Definitions and means of control variables that are included in the models are presented in Appendix Table A.1. Full results from a representative set of the impact equations that underlie these results are presented in Appendix Tables A.9 and A.10.

[a] Other cash transfers primarily include AFDC and general assistance. However, some individuals received dependent and survivor Social Security benefits, special state or local stipends for training or housing, and Unemployment Insurance benefits.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

receiving cash transfers other than SSI and SSDI and the percentage who were living in subsidized housing did not persist into month 15. In fact, for the housing outcome measure, virtually no program impact existed by month 15. Instead, the effect estimated for the percentage who were receiving SSI or SSDI is sizeable and statistically significant. Experimentals were 19 percent less likely than were controls to be receiving such transfers at that time (33 versus 41 percent). Due largely to this result, experimentals were also 14 percent less likely to be receiving any cash transfers (45 versus 52 percent).

By month 22, no estimated effects were statistically significant, and the direction of the point-estimates of the effects was not even consistently negative. However, it may be noteworthy that the estimated effect on the receipt of SSI or SSDI was still negative and relatively large, indicating that experimentals were 13 percent less likely to be receiving such benefits than were controls (35 versus 40 percent).

A further examination of these patterns of program-induced changes in public transfer dependence in the postprogram period revealed that the program impacts on the probability of receiving such transfers are directly related to changes in the incidence of employment. The average impacts on overall benefit levels offset less than 20 percent of the earnings gain, while the effect on SSI and SSDI offset over one-third of the earnings gain. Thus, it is clear that STETS not only reduced public transfers, but also tended to move some individuals off SSI and SSDI onto other forms of assistance.

C. PROGRAM EFFECTS FOR SELECTED SUBGROUPS

In view of the limited effects of STETS on the entire sample, it is appropriate to estimate some sets of program effects by subgroups to identify patterns that may be hidden in the more aggregate analysis. The outcome measures for the subgroup analysis--average monthly income from SSI or SSDI and average monthly income from other cash transfers--were selected because they were deemed to reflect changes in both the percentage of the sample who were receiving the respective types of transfers and the average monthly income of those who were receiving benefits.[1]

As shown in Tables VI.4.A and VI.4.B, no clear and persistent patterns of site differences emerge for either outcome measure. At any point in time, the experimental effects estimated for selected sites do stand out at one extreme or another. However, the pattern of effects changed dramatically over time in ways that preclude us from drawing any conclusions about site patterns.

Some patterns do emerge among other subgroups. Of course, given the modest sample sizes and the general lack of significant experimental effects even for the overall sample, all subgroup patterns must be regarded as very speculative. Among subgroups defined by IQ level, the estimated effects for those with a mild level of retardation were consistently negative for SSI/SSDI benefits, grew in absolute value over time, and became statistically significant even for the modest sample size. Reductions in other cash benefits were also estimated for months 6 and 15, but, for month 22, it is estimated that the program increased the receipt of other cash benefits. On the other hand, the effects estimated for those with moderate retardation were consistently positive for both outcome measures, and were quite large by month 22.

Estimated reductions in SSI and SSDI benefits were particularly large and consistent over time for males and blacks. Reductions estimated for younger sample members were also consistent over time and, in month 22, were larger than those that were estimated for older sample members. Reductions that are large relative to others in their respective subgroups were estimated for months 15 and 22 both for sample members who were living independently and for those with an organic cause of retardation. For some other sets of subgroups, particularly those defined by IQ level and financial management skills, some very large and occasionally significant estimated effects occurred, but without clear patterns.

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[1] The full regression results for the models from which these two sets of results were generated are shown in Appendix Tables A.9 and A.10.

TABLE VI.4

ESTIMATED PROGRAM IMPACTS ON INCOME FROM TRANSFERS FOR KEY SUBGROUPS OF STETS PARTICIPANTS

A. AVERAGE MONTHLY INCOME FROM SSI OR SSDI

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	$66.41	$74.59	$-8.18	$91.35	$109.65	$-18.30	$99.27	$120.03	$-20.76
Site									
Cincinnati	58.11	64.64	-6.53	71.64	78.08	-6.44	67.34	105.07	-37.73
Los Angeles	106.63	119.02	-12.39	180.10	186.19	-6.09	180.58	207.39	-26.81
New York	59.54	49.23	10.31	86.56	119.02	-32.46	85.42	126.11	-40.69
St. Paul	12.48	40.91	-28.43	13.15	57.24	-44.09	49.65	30.69	18.96
Tucson	76.14	91.74	-15.60	74.92	86.44	-11.52	93.74	96.90	-3.16
IQ Level									
Borderline	58.49	72.54	-14.05	82.15	81.92	0.23	89.70	83.38	6.32
Mild	64.39	72.08	-7.69	90.55	121.63	-31.08*	98.02	143.75	-45.73**
Moderate	96.17	92.07	4.10	122.54	119.68	2.86	131.85	85.16	46.69
Age									
Younger than 22	61.34	81.25	-19.91	84.05	100.76	-16.71	85.92	112.09	-26.17
22 or older	80.48	56.09	24.39	108.08	130.03	-21.95	129.21	137.86	-8.65
Gender									
Male	72.83	71.50	1.33	81.86	121.16	-39.30**	91.46	133.93	-42.47**
Female	58.09	78.59	-20.50	83.80	94.02	-10.22**	109.26	102.24	7.02
Race/Ethnicity									
Black	40.82	60.31	-19.49	52.86	108.18	-55.32**	67.30	92.50	-25.20
Hispanic	47.61	62.47	-14.86	106.82	81.69	25.13	94.36	94.58	-0.22
White and other	84.55	85.03	-0.48	110.29	118.10	-7.81	118.04	141.68	-23.64
Living Arrangement									
Living with parents	64.80	74.46	-9.66	87.12	103.54	-16.42	102.13	111.08	-8.95
Living in supervised setting	26.24	77.81	-51.57	102.65	121.92	-19.27	78.11	102.77	-24.66
Living independently	136.25	71.91	64.34	123.89	161.12	-37.23	96.81	229.20	-132.39**
Financial Management Skills									
Independent	66.30	80.21	-13.91	89.81	92.18	-2.37	92.74	122.46	-29.72
Not independent	66.46	72.33	-5.87	91.92	116.20	-24.28*	101.73	119.12	-17.39
Receipt of Transfers									
SSI/SSDI	175.17	194.81	-19.64	199.01	202.24	-3.23	203.06	180.55	22.51
Other transfers only	21.21	18.46	2.75	37.33	75.83	-38.50*	53.91	114.13	-60.22**
No transfers	8.08	14.74	-6.66	36.52	50.63	-14.11	36.12	63.66	-27.54
Cause of Retardation									
Organic	90.44	31.99	58.45*	100.70	165.43	-64.73**	92.91	186.57	-93.66**
Non-organic	61.79	82.79	-21.00	89.53	98.82	-9.29	100.56	106.52	-5.96
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	59.23	75.84	-16.61	130.99	103.28	27.71	96.19	141.25	-45.06
Other job lasting >3 months	76.62	84.25	-7.63	79.68	102.22	-22.54	106.51	87.67	18.84
Other	61.35	67.45	-6.10	88.13	116.30	-28.17	95.34	135.51	-40.17**
School Status at Referral									
Enrolled	71.70	72.24	-0.54	114.98	124.23	-9.25	116.55	132.02	-15.47
Not enrolled	64.04	75.65	-11.61	80.39	102.89	-22.50	91.36	114.55	-23.19
Number in Sample			287			408			399

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that were included in the models which underlie the overall net impact estimates reported in Table VI.3, these models include variables that interacted the treatment variable with the subgroup variables. Full results from the regression model for the month 6 and month 22 outcomes are presented in Appendix Tables A.9 and A.10.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

TABLE VI.4

ESTIMATED PROGRAM IMPACTS ON INCOME FROM TRANSFERS FOR KEY SUBGROUPS OF STETS PARTICIPANTS

B. AVERAGE MONTHLY INCOME FROM OTHER CASH TRANSFERS[a]

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	$13.04	$22.42	$-9.38	$22.26	$29.71	$-7.45	$29.23	$19.45	$9.78
Site									
Cincinnati	2.99	21.50	-18.51	14.48	45.34	-30.86	40.11	27.08	13.03
Los Angeles	0.42	0.11	0.31	5.11	4.78	0.33	19.50	7.01	12.49
New York	19.80	36.68	-16.88	26.63	18.01	8.62	25.95	10.70	15.25
St. Paul	73.00	67.96	5.04	82.61	89.76	-7.15	62.64	40.08	22.56
Tucson	(6.24)	2.87	-9.11	6.77	17.40	-10.63	13.16	21.71	-8.55
IQ Level									
Borderline	8.86	15.14	-6.28	30.28	25.37	4.91	28.59	23.20	5.39
Mild	12.70	26.81	-14.11	14.22	31.85	-17.63	27.58	20.17	7.41
Moderate	24.77	19.52	5.25	44.76	29.66	15.10	39.73	6.17	33.56
Age									
Younger than 22	16.32	16.37	-0.05	27.07	26.58	0.49	34.38	12.09	22.29**
22 or older	4.21	38.68	-34.47**	11.41	36.76	-25.35	17.99	35.53	-17.54
Gender									
Male	7.89	27.84	-19.95*	7.75	26.62	-18.87	19.73	14.01	5.72
Female	19.77	15.32	4.45	42.21	33.96	8.25	41.74	26.62	15.12
Race/Ethnicity									
Black	6.27	15.92	-9.65	35.43	20.30	15.13	51.85	34.91	16.94
Hispanic	14.57	17.19	-2.62	19.58	38.82	-19.24	12.92	20.77	-7.85
White and other	16.55	27.07	-10.52	15.14	32.89	-17.75	20.70	10.40	10.30
Living Arrangement									
Living with parents	13.68	21.87	-8.19	26.44	32.25	-5.81	29.12	18.75	10.37
Living in supervised setting	10.88	47.74	-36.86	(8.56)	34.61	-43.17	22.25	41.16	-18.91
Living independently	8.44	-2.80	11.24	15.59	-2.25	17.84	38.65	-0.04	38.69
Financial Management Skills									
Independent	14.18	41.80	-27.62*	25.52	50.17	-24.65	40.31	42.58	-2.27
Not independent	12.59	14.78	-2.19	21.06	22.17	-1.11	25.08	10.79	14.29
Receipt of Transfers									
SSI/SSDI	15.06	13.96	1.10	35.07	25.53	9.54	29.55	13.85	15.70
Other transfers only	29.43	62.71	-33.28**	20.44	52.40	-31.96*	30.94	37.40	-6.46
No transfers	(1.11)	-0.75	-0.36	11.46	12.03	-0.57	27.24	7.77	19.47
Cause of Retardation									
Organic	15.42	24.73	-9.31	6.21	14.65	-8.44	24.29	10.21	14.08
Non-organic	12.56	21.96	-9.40	25.49	32.75	-7.26	30.26	21.38	8.88
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	(3.76)	20.08	-23.84	7.74	70.88	-63.14**	12.59	42.16	-29.57
Other job lasting >3 months	21.17	22.24	-1.07	39.86	9.24	30.62*	46.71	10.03	36.68**
Other	12.15	23.25	-11.10	14.50	31.96	-17.46	22.05	19.42	2.63
School Status at Referral									
Enrolled	10.18	32.83	-22.65	13.31	36.81	-23.50	34.46	29.32	5.14
Not enrolled	14.32	17.74	-3.42	26.34	26.48	-0.14	26.83	14.92	11.91
Number in Sample			284			413			401

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table VI.3, these models include variables that interacted the treatment variable with the subgroup variables. Full results from the regression models for the month 6 and month 22 outcomes are presented in Appendix Tables A.9 and A.10.

[a] Other cash transfers primarily include AFDC and general assistance. However, some individuals received dependent and survivor Social Security benefits, special state or local stipends for training or housing, and Unemployment Insurance benefits.

*Statistically significant at the 10 percent level, two-tailed test.

**Statistically significant at the 5 percent level, two-tailed test.

The trends are quite different for cash transfers other than SSI and SSDI: the effects estimated for older sample members and Hispanics were more consistently negative and generally larger than those estimated for others in the respective sets of subgroups. Females, sample members who were living independently, and, after month 6, blacks showed evidence that their receipt of these other cash transfers increased--patterns that sometimes, but not always, parallel the earnings results for these subgroups, reported in Appendix Table A.4.B.

D. SUMMARY AND CONCLUSIONS

The evaluation of the impacts of STETS on cash and in-kind transfer dependence began with a strong hypothesis--that, conditional on the impacts of the program on earnings, STETS participation would reduce the receipt and, for cash transfers, the amount of transfers. While impacts on transfers were expected to lag behind impacts on earnings, it seemed likely that they would have occurred by the end of the observation period, given the pattern of earnings impacts. However, the results generally did not show this expected pattern of effects. In fact, the impacts that were observed occurred early and then tended to fade. The one encouraging sign is that the pattern for SSI and SSDI--point-estimates of reductions in both the receipt and amount of SSI or SSDI--persisted over time. In month 22, these reductions offset 38 percent of the total earnings gain. While the estimated effects are generally not statistically significant at the conventionally accepted levels, the point estimates are reasonably large. We believe that a useful practice would be to follow these SSI/SSDI-related outcomes for a longer period of time, perhaps by using Social Security records data.

VII. IMPACTS ON ECONOMIC STATUS, INDEPENDENCE, AND LIFE-STYLE

As was discussed in Chapter IV, the STETS program had a substantial impact on the incidence of regular job-holding and on the work hours in and earnings from those jobs. It might be expected that the STETS demonstration would also have impacts on other areas of participants' lives--especially their overall economic status, their independence in financial management and living arrangement, their use of formal and informal services, and their general level of involvement in regular, productive activities.

However, the expected direction and duration of the effects of the program on economic status, measures of independence, and life-style are not always clear. Several factors in particular cloud the results of any present evaluation of these impacts. First, the increased earnings observed for experimentals appear to be offset partially by decreases in transfer benefits and other sources of income, thereby diluting the overall financial impacts of the program. Second, although STETS may have had impacts on financial management skills and independent living arrangements, those impacts may follow others with a considerable time delay, in which case the 22-month observation period of our study may be too short to observe such program impacts. Third, although the program generated increased earnings for sample members, those increases might not have been enough to enable them to live independent life-styles--especially in such large metropolitan areas as New York or Los Angeles. Finally, parents and counselors might simply wish to see more concrete and stable earnings gains before they are willing to give the sample members greater independence.

Despite these limitations in our ability to detect what may be primarily long-run effects, we have pursued an analysis of such impacts due to the strong policy interest in understanding the effects of transitional-employment programs such as STETS on the life-styles of participants, in addition to or because of its success in integrating mentally retarded individuals into the workplace. To varying levels of detail, as permitted by the availability and quality of available data, this chapter explores the impacts of STETS on the following outcomes:

- Personal income from all sources
- Independence in financial management
- Use of formal services
- Existence of a personal relationship with a benefactor
- Involvement in some regular activity which could lead to integration into the labor market
- Living arrangement
- Family status

As in the previous chapters, we begin by providing an overview of the experience of the control group to describe how STETS participants would have fared in the absence of the demonstration. In Section B, we discuss the impacts of the program on specific outcome measures. In the final section, we summarize the main findings and discuss their policy implications.

A. EXPERIENCE IN THE ABSENCE OF STETS

The experience of control group members in terms of economic status, independence, and life-style reflect the expected experience of the experimental group had they not enrolled in the STETS program.

1. Economic Status

Weekly personal income was measured as the sum of all earnings, transfer program benefits, and any other regular sources of income per week. As shown in Table VII.1, average weekly personal income from all sources varied considerably across sites (from $27 in Cincinnati to $57 in Los Angeles at baseline, and from $29 in Cincinnati to $79 in New York at month 22). However, even at its highest level, the average total income of the STETS control group ($66 per week) was only about 60 percent of the federal poverty-income guidelines for family units of one, which were $77.11 per week at the start of the demonstration, 1982, $93.46 in 1983,

TABLE VII.1

WEEKLY PERSONAL INCOME FROM ALL SOURCES FOR CONTROL GROUP MEMBERS, BY SITE

	Site					
Time Period	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Total Dollars of Personal Income						
Baseline[a]	$26.61	$57.28	$41.52	$43.64	$49.43	$43.54
Month 6	31.53	43.59	65.43	48.63	55.90	49.63
Month 15	30.64	53.88	86.32	65.12	56.74	58.24
Month 22	28.75	56.81	79.27	63.75	71.44	60.69
Number In Sample:						
Baseline	39	36	48	26	44	193
Month 6	28	27	32	17	29	133
Month 15	44	38	45	26	42	195
Month 22	39	36	45	25	44	189

NOTE: Sources for total personal income include earnings from jobs, transfer-program (SSI, SSDI, and welfare) benefits, Social Security benefits, stipends or grants, state supplements for residential support, and regular contributions by parents, based upon reports by primary sample members or proxies.

[a] In some cases, the baseline interviews were administered several weeks after random assignment.

and $97.77 at the end of the follow-up period, 1984.[1] Had these young people been living on their own (which, by and large, they were not), their general economic status would have been very low.

In general, average personal income among the entire control group did increase substantially over time, from $44 per week at baseline to $61 per week at month 22 (39 percent). However, all of this increase is attributable to increases among the New York, St. Paul, and Tucson samples--90, 46, and 45 percent, respectively. In Cincinnati, total personal income remained low throughout the demonstration ($27 to $31 per week), and although the control group in Los Angeles had the highest average personal income at the start of the demonstration ($57 per week) they experienced virtually no increase over the 22-month follow-up period.

2. Independence and Life-Style

Independence and life-style encompass a variety of concepts, only a few of which could be analyzed within the context of this evaluation. The specific concepts considered below include financial management skills, the use of support services, personal relationships with benefactors, the extent of inactivity, and living arrangement.

Financial Management Skills. The interviews provided data on whether sample members received assistance from anyone in three areas of financial management--paying sales clerks when shopping, handling bills (i.e., arranging for the payment of but not necessarily providing the funds for bills), and transferring money in or out of bank accounts. Those who performed at least two of the three activities without assistance (and received no assistance on the third) were considered to exhibit financial management skills. As shown in Table VII.2, between 25 and 35 percent of the control group members exhibited financial management skills at each

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[1] These figures are reported in the following three respective citations of the Federal Register: Vol. 49, No. 39, February 27, 1984, p. 15418; Vol. 48, No. 34, February 17, 1983, p. 7010; and Vol. 49, No. 39, February 27, 1984, p. 7152.

TABLE VII.2

SOCIAL SUPPORT AND INDEPENDENCE OF CONTROL GROUP MEMBERS, BY SITE

Type and	Site					
Time Period	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent Who Demonstrate Independence in Financial Management[a]						
Baseline[b]	25.6	26.2	35.3	18.5	17.0	25.2
Month 6	27.6	22.2	27.0	38.9	32.3	28.9
Month 15	25.0	33.3	41.7	25.9	40.9	34.2
Month 22	28.2	31.6	47.8	25.9	34.1	34.5
Percent Receiving Any Service from Agency[c]						
Baseline	84.1	92.9	88.2	92.6	93.6	90.0
Month 6	65.5	92.6	73.0	83.3	80.6	78.2
Month 15	68.2	69.2	54.2	85.2	72.7	68.3
Month 22	66.7	73.7	45.7	74.1	72.7	65.5
Percent Receiving Job Training Services						
Baseline	16.7	40.5	44.9	14.8	27.9	30.5
Month 6	20.7	44.4	44.4	50.0	38.7	39.0
Month 15	25.0	28.2	29.2	46.2	27.3	29.9
Month 22	23.7	26.3	21.7	42.3	38.6	29.7
Percent Receiving Job Search Assistance						
Baseline	29.5	35.7	43.1	55.6	30.4	37.6
Month 6	17.2	23.1	22.2	17.6	25.8	21.6
Month 15	15.9	15.4	8.3	11.1	20.5	14.4
Month 22	17.9	13.2	15.2	11.1	11.6	14.0
Percent Receiving School Services						
Baseline	46.5	16.7	37.3	11.1	51.1	34.8
Month 6	17.2	29.6	21.6	11.1	12.9	19.0
Month 15	9.1	7.7	14.6	0.0	18.2	10.9
Month 22	7.7	18.4	8.7	3.7	20.5	12.4
Percent Receiving Residential Counseling						
Baseline	6.8	9.5	0.0	29.6	10.6	9.5
Month 6	6.9	22.2	0.0	27.8	6.5	10.6
Month 15	4.5	10.3	0.0	37.0	11.4	10.4
Month 22	7.7	13.2	0.0	25.9	6.8	9.3
Percent Receiving Other Counseling						
Baseline	45.5	69.0	56.9	63.0	68.1	60.2
Month 6	34.5	66.7	54.1	55.6	61.3	54.2
Month 15	54.5	51.3	35.4	63.0	54.5	50.5
Month 22	51.3	60.5	37.0	51.9	63.6	52.6
Percent Receiving Assistance with Financial Management[d]						
Baseline	7.0	14.3	2.0	22.2	17.0	11.4
Month 6	6.9	22.2	8.1	11.1	16.1	12.7

TABLE VII.2 (continued)

Type and Time Period	Site					Total
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	
Month 15	6.8	7.7	2.1	29.6	9.1	9.4
Month 22	5.1	18.4	2.2	29.6	11.4	11.9
Percent Receiving Transportation Assistance[e]						
Baseline	20.9	7.1	2.0	3.7	10.6	9.0
Month 6	10.3	7.4	0.0	16.7	9.7	7.7
Month 15	13.6	7.7	4.2	3.7	6.8	7.4
Month 22	20.5	2.6	4.3	3.7	9.1	8.2
Percent with Benefactor[f]						
Baseline	13.6	28.6	27.5	40.7	46.8	30.8
Month 6	17.2	44.4	21.6	16.7	22.6	24.6
Month 15	13.6	23.1	6.3	33.3	31.8	20.3
Month 22	10.3	13.2	13.0	25.9	27.3	17.5
Number in Sample[g]						
Baseline	44	42	51	27	47	211
Month 6	29	27	37	18	31	142
Month 15	44	39	48	27	44	202
Month 22	39	38	46	27	44	194

[a] Independence is defined as performing without assistance at least two financial management activities (shopping, handling bills, and using bank accounts), and without assistance in the other financial management activity.

[b] In some cases, baseline interviews were administered several weeks after random assignment.

[c] The services that were provided by agency staff members included job training, job-search assistance, schooling, residential counseling, other counseling, financial management (assistance with shopping, bill paying, or handling bank accounts, or receiving transfer-program benefits on behalf of the individuals), and transportation (providing regular transportation to job, training program, or school).

[d] This heading is defined as assistance with shopping, handling bills, using bank accounts, or receiving transfer-program benefits on behalf of the individual.

[e] This heading is defined as providing regular transportation to a job, training program, or school.

[f] Benefactors are individuals named by primary sample members as providing assistance in two or more of the following areas: job search, residential counseling, other counseling, financial management, and transportation. These individuals could be relatives or friends of the sample member, or service agency staff members.

[g] Sample sizes for individual outcomes varied due to missing data. The numbers for the total sample ranged from 203 to 211 at baseline, from 141 to 142 at month 6, from 201 to 202 at month 15, and from 192 to 194 at month 22.

interview wave. Among those who did not, most either had only one financial management opportunity which they performed independently or had opportunities in two or more financial management activities but were dependent in at least one activity; very few exhibited total dependence in financial management or had no opportunities to exercise financial management skills.

Over the 22-month period, controls in Cincinnati experienced the smallest gains in the percentage who exhibited financial management skills (3 percentage points), while those in Tucson experienced the largest gains (17 percentage points). Only in New York did almost half of the control group members demonstrate financial management skills by month 22. These relatively low levels of independence may be attributable in part to the low levels of income available to the individuals and/or to the fact that most continued to live with their parents or other responsible adults.

Service Receipt. The control group had access to and received a variety of services throughout the demonstration period. As shown in Table VII.2, and as was expected given that most sample members were referred to the program through the service network, service receipt was particularly high at baseline (90 percent), although service use declined over the follow-up period. (In month 22, 65 percent of the sample still maintained contact with a service agency.) By far the most commonly used type of service was counseling outside of residential settings, which was used by 50 to 60 percent of the control group at each time period.

As was noted in Chapter V, about 30 percent of the control group received job training. Although variations occurred across the five sites in the use of job-training, the overall average level of use remained generally the same through month 22. While about one-third of the controls also used job-search assistance and schooling services at baseline, the extent to which they used these services tended to drop off fairly quickly, to less than 15 percent by month 22.

Only a small proportion of the control group (9 to 13 percent) used the remaining types of services--residential counseling, assistance with financial management, and transportation assistance. However, great variations occurred across the sites in the level of such use. For example, generally over 25 percent of the control group in St. Paul received residential counseling and assistance with financial management, compared with the overall sample average of approximately 10 percent. The control group in Cincinnati was twice as likely as the entire sample to receive transportation services at baseline and at month 22. These variations reflect differences in the types of support services available in the demonstration communities, as well as differences in individual needs for particular services.

Personal Relationships with Benefactors. A much smaller proportion of the control group (31 percent) had a personal relationship with a benefactor at the time they were referred to STETS.[1] Control group members in St. Paul and Tucson were the most likely to have had a personal relationship with a benefactor (41 and 47 percent, respectively), while those in Cincinnati were the least likely to have had one (14 percent). In all sites, personal relationships with benefactors waned over time; only 18 percent reported having such relationships by month 22.

Degree of Inactivity. As we discussed in Chapter IV, most control group members were involved in some activity pertaining to employment or training. As shown in Table VII.3, nearly 40 percent were inactive in each time period--that is, they were not employed at least 4 hours a week and were not enrolled in a training or school program. In general, the activity status of the control group did not change substantially over time--with the exception of the control group in St. Paul (where the percentage who were inactive decreased by 63 percent) and the control group in Cincinnati (where percentage who were inactive increased by 79 percent).[2] The substantial proportions of controls who were inactive

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[1] Benefactors included relatives, friends, or agency staff members who provided assistance in two or more of the following key areas: job search, residential counseling, other counseling, financial management, and transportation.

[2] The decrease in St. Paul appears to have been due to the greater number who participated in training (particularly in workshops and activity centers), while the increase in Cincinnati appears to have been due to the greater number who left schooling programs (see Chapters IV and V).

TABLE VII.3

PERCENT OF CONTROL GROUP MEMBERS WHO WERE INACTIVE, BY SITE

	Site					
Time Period	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent Inactive[a]						
Baseline[b]	37.2	45.2	35.3	59.3	23.9	38.3
Month 6	55.2	29.6	21.6	33.3	35.5	34.5
Month 15	61.4	53.8	16.7	29.6	31.8	38.6
Month 22	66.7	47.4	32.6	22.2	25.2	39.2
Number in Sample:						
Baseline	43	42	51	27	46	209
Month 6	29	27	37	18	31	142
Month 15	44	39	48	27	44	202
Month 22	39	38	46	27	44	194

[a] Not in any paid or unpaid job of at least 4 hours per week, or in a training or school program.

[b] In some cases, baseline interviews were administered several weeks after random assignment.

suggests that, without such programs as STETS, a high proportion of mentally retarded young adults are at risk of remaining inexperienced in activities designed to foster employability.

Living Arrangement.[1] Like many young adults, high proportions (78 to 84 percent) of the control group members in the STETS sample lived with their parents, foster parents, or other adult relatives (see Table VII.4). Only in St. Paul and Tucson were substantially higher-than-average proportions of controls living in other settings. Over time, the proportion of controls who were living with their parents tended to decrease only slightly in all sites.

In general, the percentages who were living in supervised settings (group homes, supervised apartments, and institutions) remained fairly constant (about 10 percent), although these percentages ranged from 2 percent or less in New York to over 26 to 30 percent in St. Paul. This variation across sites may reflect differences in the availability of supervised-living units, the different methods whereby the sample members were recruited from those living in such settings,[2] and/or individual differences in the degree of independence. Modest increases occurred in the percentage of controls who lived independently (either alone or with non-family-related roommates), from 7 percent at baseline to 13 percent at the 22-month interview. This increase parallels the small movement away from living with parents.

The STETS control group generally did not begin to establish their own families within the 22 months of observation. Only two persons lived

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[1] In addition to the living arrangements discussed in this section, we also considered the prevalance of incarceration among sample members, since this measure provides our only evidence on the impacts of the program on anti-social behavior. The incidence of incarceration was sufficiently low overall (one control at the 6-month interview and three at each of the 15- and 22-month interviews) that no real conclusions can be drawn from these data.

[2] For example, the St. Paul program recruited heavily (80 percent) from the state vocational rehabilitation agency (see Table 2.2 of Riccio, 1984), which may have also arranged for placements in supervised residential settings.

TABLE VII.4

LIVING ARRANGEMENTS OF CONTROL GROUP MEMBERS, BY SITE

Type and	Site					
Time Period	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total
Percent Living with Parents or Foster Parents						
Baseline[a]	90.9	84.1	94.6	66.7	73.5	83.6
Month 6	93.1	74.1	100.0	72.2	80.7	85.9
Month 15	81.8	79.5	93.6	51.9	70.5	77.6
Month 22	87.2	78.9	89.1	59.3	68.2	77.8
Percent Living in Supervised Setting[b]						
Baseline[a]	9.1	6.8	1.8	25.9	10.6	9.1
Month 6	6.9	22.2	0.0	27.8	6.5	10.6
Month 15	6.8	10.3	2.1	29.6	11.4	10.5
Month 22	7.7	10.5	2.2	25.9	6.8	9.3
Percent Living Independently[c]						
Baseline[a]	0.0	9.1	3.6	7.4	16.2	7.3
Month 6	0.0	3.7	0.0	0.0	12.9	3.5
Month 15	11.4	10.3	4.3	18.5	18.2	11.9
Month 22	5.1	10.5	8.7	14.8	25.0	12.9
Number in Sample:						
Baseline	44	44	56	27	49	220
Month 6	29	27	37	18	31	142
Month 15	44	39	47	27	44	201
Month 22	39	38	46	27	44	194

[a] In some cases, baseline interviews were administered several weeks after random assignment.

[b] This heading includes group homes, supervised apartments, and institutions.

[c] This heading includes living alone, living with unrelated roommates, and living with spouse and/or own children (but not with other related adults).

with their own children at baseline, and none lived with a spouse; by month 22, the number who were living with their own children had increased to seven, and a total of eight were living with a spouse.

B. ESTIMATED PROGRAM IMPACTS ON ECONOMIC STATUS, INDEPENDENCE, AND LIFE-STYLE

The analytic techniques that underlie our evaluation of the program impacts addressed in this chapter vary according to both the nature of the program outcome measures and the distribution of the sample in terms of the measures. For example, personal income, independence in financial management, the use of support services, and inactivity were analyzed with ordinary least squares (OLS) regression techniques. However, because living arrangement (a three-category variable in which approximately 80 percent of the sample falls into one category) is not amenable to such techniques, we instead present comparisons of unadjusted percentages of this outcome. Similarly, family status variables have such skewed distributions that, with the relatively small sample sizes, tests of statistical significance would be highly unreliable regardless of the technique used. Thus, the results for these variables are discussed only briefly and for descriptive purposes.

1. Program Impacts on Economic Status

Table VII.5 presents the program impact estimates on weekly personal income, along with the regression-adjusted mean values for both the experimental and control groups.[1] In terms of in-program effects (i.e., the month 6 results), the results suggest that the STETS intervention significantly increased the total income of the experimental group by $21 per week. Relatively little variation occurred in the estimated size of the income effects in this time period among the various sample subgroups. The noteworthy exceptions are that larger-than-average estimated

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[1] Experimental-control differences in the percentage distributions of sample members by income level are presented in Appendix Table A.14. These results indicate that the income gains associated with program participation accrued to individuals at all income levels.

TABLE VII.5

ESTIMATED PROGRAM IMPACTS ON TOTAL DOLLARS OF WEEKLY PERSONAL INCOME, FOR TOTAL SAMPLE AND FOR KEY SUBGROUPS OF STETS PARTICIPANTS

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	71.72	50.94	20.78**	67.22	59.67	7.55	71.59	62.39	9.20
Site									
Cincinnati	59.32	41.59	17.73	47.85	39.13	8.72	49.84	33.77	16.07
Los Angeles	67.98	41.89	26.09*	69.47	51.92	17.55	75.76	59.66	16.10
New York	90.80	69.01	21.79	83.97	88.27	-4.30	88.96	84.59	4.37
St. Paul	63.85	48.85	15.00	63.61	66.74	-3.13	76.45	59.47	16.98
Tucson	70.00	49.18	20.82	66.70	51.11	15.59	65.85	68.46	-2.61
IQ Level									
Borderline	73.20	45.13	28.07**	65.37	61.70	3.67	75.59	58.00	17.59
Mild	69.56	52.29	17.27**	62.54	58.21	4.33	66.21	67.31	-1.10
Moderate	78.23	58.63	19.60	104.88	66.23	38.65**	89.95	46.55	43.40**
Age									
Younger than 22	72.77	50.13	22.64**	68.68	55.70	12.98*	68.84	60.98	7.86
22 or older	68.89	53.15	15.74	63.85	68.76	-4.91	77.81	65.57	12.24
Gender									
Male	76.03	48.70	27.33**	72.07	64.57	7.50	80.41	63.46	16.95**
Female	66.07	53.88	12.19	60.69	53.07	7.62	60.23	61.00	-0.77
Race/Ethnicity									
Black	66.52	42.33	24.19**	54.06	52.47	1.59	63.34	55.79	7.55
Hispanic	82.91	51.10	31.81	87.63	75.98	11.65	87.20	64.85	22.35
White and other	72.50	55.74	16.76*	69.34	59.41	9.93	72.06	65.35	6.71
Living Arrangement									
Living with parents	69.69	52.87	16.82**	65.78	59.84	5.94	73.38	59.63	13.75**
Living in supervised setting	65.97	60.57	5.40	62.42	62.54	-0.12	55.94	69.55	-13.61
Living independently	100.78	18.19	82.59**	86.92	54.88	32.04	72.50	81.26	-8.76
Financial Management Skills									
Independent	74.57	64.89	9.68	88.51	63.76	24.75**	91.76	68.78	22.98*
Not independent	70.57	45.29	25.28**	58.99	58.09	0.90	63.90	59.95	3.95
Receipt of Transfers									
SSI/SSDI	100.06	77.81	22.25*	90.69	78.56	12.13	98.20	72.80	25.40**
Other transfers only	68.62	45.26	23.36*	59.05	54.18	4.87	69.86	60.15	9.71
No transfers	50.26	32.70	17.56*	51.06	45.46	5.60	46.39	54.04	-7.65
Cause of Retardation									
Organic	72.23	47.78	24.45	67.04	56.54	10.50	78.37	67.94	10.43
Non-organic	71.62	51.57	20.05**	67.25	60.26	6.99	70.21	61.26	8.95
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	77.99	55.34	22.65	87.63	79.62	8.01	84.84	84.45	0.39
Other job lasting >3 months	74.88	53.09	21.79**	68.46	58.89	9.57	71.99	62.12	9.87
Other	67.38	47.95	19.43**	60.58	54.47	6.11	67.50	56.22	11.28
School Status at Referral									
Enrolled	66.63	42.49	24.14*	59.67	58.56	1.11	66.86	59.19	7.67
Not enrolled	73.96	54.66	19.30**	70.56	60.16	10.40	73.76	63.86	9.90
Number in Sample			274			391			392

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the model which underlies the overall net impact estimates reported for the total sample (see Appendix Table A.1), the model include variables that interacted the experimental status variable with the subgroup variables. The number of cases in each subgroup is reported in Appendix Table A.12

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

effects occurred for males, blacks and Hispanics, and individuals under the age of 22--groups that have traditionally exhibited high youth unemployment rates, even without intellectual or other disabilities. Other groups which experienced large in-program effects were those who were living independently at baseline and those whose IQ scores indicate borderline retardation.

For the postprogram period, a different pattern of results is observed. Estimated impacts on personal income are no longer large or statistically significant for the total sample; the point estimate of the overall effect in month 22 is $9.20 per week. The only subgroups which showed sustained evidence of income gains at months 15 and 22 were those with moderate retardation, males, Hispanics, those who were living with parents, those who exhibited independence in financial management skills at baseline, and those who received SSI/SSDI at referral. With the exception of the SSI/SSDI recipients and the moderately retarded individuals, each of these subgroups experienced earnings gains that roughly equalled these total income effects (see Appendix Table A.4). For the moderately retarded and the SSI/SSDI recipients, the impacts are attributable to sizeable effects on both earnings and SSI/SSDI (see Table VI.4 in Chapter VI and Appendix Table A.4).

2. Program Impacts on Independence and Life-Style

The estimated impacts of STETS on independence in financial management and the use of social support are generally positive but small (see Table VII.6). By month 22, virtually no difference between the experimental and control groups existed in terms of independence in financial management, the receipt of formal services, or personal relationships with benefactors. The only noteworthy impact is the increased percentage who used formal services (from 79 to 88 percent) during the in-program period (month 6), which is likely to be a direct result of participating in STETS.

Similarly, STETS had no impact on whether sample members were inactive, except during the period of program participation itself. As we indicated in Chapters IV and V, a number of offsetting impacts on activities occurred in the postprogram period. While the experimental

TABLE VII.6

ESTIMATED PROGRAM IMPACTS ON SOCIAL SUPPORT, INDEPENDENCE, AND INACTIVITY

	Month 6			Month 15			Month 22		
Outcome Measures	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Percent Who Demonstrate Independence in Financial Management[a]	36.9	29.1	7.8	37.5	35.2	2.3	36.3	35.5	0.8
Percent Receiving Any Services From Agency[b]	87.9	78.7	9.2**	70.5	68.1	2.4	68.2	65.3	2.9
Percent With Benefactor[c]	31.6	30.2	1.4	31.1	22.6	8.5*	24.7	19.8	4.9
Percent Inactive[d]	20.1	36.2	-16.1**	42.3	38.2	4.1	41.3	39.7	1.6
Number in Sample			288			416			404

NOTE: These results were estimated through ordinary least squares techniques. Definitions and means of control variables that are included in the models are presented in Appendix Table A.1.

[a] Independence is defined as performing without assistance at least two financial management activities (shopping, handling bills, and using bank accounts), and without assistance in the other financial management activity.

[b] The services that were provided by agency staff members included job training, job-search assistance, schooling, residential counseling, other counseling, financial management (assistance with shopping, bill paying, or handling bank accounts, or receiving transfer-program benefits on behalf of the individuals), and transportation (providing regular transportation to job, training program, or school).

[c] Benefactors are individuals named by primary sample members as providing assistance in two or more of the following areas: job search, residential counseling, other counseling, financial management, and transportation. These individuals could be relatives or friends of the sample member, or service agency staff members.

[d] Not in any paid or unpaid job of at least 4 hours per week, or in a training or school program.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

group was more likely than controls to hold a regular job, fewer were enrolled in training or schooling programs. Once STETS participation was over, experimental group members were no more likely to be involved in some form of employment, training, or educational activity than they would have been had they not participated in STETS.

3. Impacts on Living Arrangement

For three reasons, we assess the impacts of STETS on living arrangement by correlating percentages in various living arrangements at baseline with those at each of the follow-up periods: (1) living arrangement is a three-category outcome measure; (2) most of the sample members lived with their parents at baseline; and (3) experimental-control differences in living arrangements existed at baseline. As shown in Table VII.7, these tabulations show no evidence that STETS had a strong or consistent impact on the proportion of the sample who were living with their parents or with other adult relatives during the in-program period; in month 6, most of those who had been living at home at baseline continued to do so, and approximately equal proportions from both the experimental and control groups who had been living elsewhere had moved in with their parents by month 6. There is evidence that STETS descreased the likelihood of living in supervised settings during this in-program period, in that more experimentals than controls moved out of such arrangements, and more control group members than experimentals entered them. An estimated positive in-program impact of STETS on living independently was due to a different pattern of movements--more of the experimental group who had been independent at baseline remained independent, and more moved from other arrangements into independent living arrangements.

The postprogram results show that most of the sample continued to live in the same arrangements as at baseline. This finding was especially true for those who were living with their parents, but less so for those who were living in supervised settings. STETS did have some impact on the pattern of movements that occurred; the most substantial of these impacts occurred by month 15. At month 15, experimental group members were more likely than those in the control group to leave independent living arrange-

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TABLE VII.7

ESTIMATED PROGRAM IMPACTS ON LIVING ARRANGEMENT, BY LIVING ARRANGEMENTS AT BASELINE

A. Percent Living with Parents

	Month 6			Month 15			Month 22		
Living Arrangement at Baseline	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Living with Parents	97.4	95.2	2.2	88.1	89.5	-1.4	88.3	88.4	-0.1
Living in Supervised Setting[a]	11.8	10.0	1.8	13.0	5.9	7.1	30.4	16.7	13.7
Living Independently[b]	13.3	16.7	-3.4	27.3	9.1	18.2	21.7	27.3	-5.6
Number in Sample			236			313			307

B. Percent Living in Supervised Setting[a]

	Month 6			Month 15			Month 22		
Living Arrangement at Baseline	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Living with Parents	0.9	4.0	-3.1	5.9	4.1	1.8	5.5	4.3	1.2
Living in Supervised Setting[a]	70.6	90.0	-19.4	65.2	76.5	-11.3	43.5	61.1	-17.6
Living Independently[b]	0.0	16.7	-16.7	13.6	0.0	13.6	8.7	0.0	8.7
Number in Sample			28			48			39

C. Percent Living Independently[b]

	Month 6			Month 15			Month 22		
Living Arrangement at Baseline	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Living with Parents	1.7	0.8	0.9	5.9	6.4	-0.5	6.1	7.3	-1.2
Living in Supervised Setting[a]	17.7	0.0	17.7	21.7	17.7	4.0	26.1	22.2	3.9
Living Independently[b]	86.7	66.7	20.0	59.1	90.9	-31.8	69.6	72.7	-3.1
Number in Sample			23			52			56

NOTE: These results are based on cross-tabulations of living arrangement at baseline with those at each follow-up observation period. Sample sizes were too small to permit chi-square tests of statistical significance.

[a] This heading includes group homes, supervised apartments, and institutions.

[b] This heading includes living alone, living with unrelated roommates, and living with spouse and/or own children (but not with other related adults).

ments, either to return home or to live in a supervised setting; by month 22, the experimental and control groups exhibited few differences in terms of the percentages who were making these types of moves. More experimental than control group members moved away from supervised settings at both postprogram observations, and experimentals were more likely than controls to move from these settings into the homes of their parents.

The program had no apparent effects on family formation. By month 22, nine experimentals and eight controls were living with a spouse, and six experimentals and seven controls were living with their own children.

C. SUMMARY AND CONCLUSIONS

Our discussion of the impacts of the STETS program in previous chapters focused on those areas of independence that pertain directly to employment. We found that participation in a transitional-employment program increased employment in the competitive sector, which in itself is a move toward a more independent life-style. It also increased earnings, while somewhat decreasing dependence on cash transfer programs.

In this chapter, we observed some relatively small program effects on such measures of independence as overall economic status, services received from community agencies, and involvement in activities oriented toward employment. However, these effects generally declined to a great extent in the postprogram period, seemingly due to two factors. First, in the later observation periods, either the direct effects of STETS participation on such outcomes as total income, service utilization, and level of inactivity either were no longer evident or, where they were evident (as with personal income), the estimated effects were not statistically significant. Second, while the STETS experience provided a head start toward independence for many sample members, those who did not participate in the program also began to achieve similar levels soon afterwards. Although certain subgroups (for example, Hispanics and those with a moderate level of retardation) did seem to continue to benefit from the program, at least in terms of personal income, even those who were more likely to achieve and maintain positive effects from their experience in STETS exhibited relatively low levels of independence by the end of the observation period: most of the mentally retarded young adults in the study had total personal incomes that were less than the poverty level, thereby restricting their opportunities for achieving a more economically self-sufficient life-style. Possibly because of their low incomes, most continued to live with their parents and to exercise little independence in financial management. Finally, a substantial proportion of both the experimental and control groups were not involved in employment, school, or training, and thus had limited opportunities to gain valuable work experience and skills.

Given the short postprogram period for which we have data, we cannot tell whether a more economically and socially independent life-style would eventually be achieved by these mentally retarded young adults, or whether the effects of participating in a transitional-employment program would become more evident at a later period.

VIII. BENEFITS AND COSTS OF STETS

As we mentioned in Chapter I, a key policy issue behind the STETS demonstration was whether the impacts of the program would be sufficiently large to justify its costs. In this chapter, we examine this issue by comparing the impacts discussed in the previous chapters with estimates of the program costs. This analysis provides a framework for assessing the success of STETS and for interpreting the various impact estimates.

Benefit-cost analysis uses dollar values as a common denominator to draw comparisons between the diverse program impacts and costs. This analysis assigns dollar values to the impacts, sums the resulting values, and compares them with the cost estimates. In doing so, it inevitably excludes some impacts, either because evaluation resources were insufficient to enable us to measure them or because the impacts are essentially intangible. In other cases, impacts were measured but were difficult to appraise and value precisely. To deal with these problems, we used a comprehensive accounting framework that lists all the major benefits and costs regardless of whether we measured them. This framework facilitates an analysis of the extent to which we have measured and included the important benefits and costs, and whether reasonable conclusions can be drawn on the basis of these measured items. We also made several alternative estimates of the value of impacts that were measured; each alternative was based on alternative assumptions about the appropriate value that should be assigned to specific impacts. This set of plausible alternatives provides a far better picture of the overall success of STETS than would a single, inherently imprecise, benefit-cost comparison.

The result of this analysis is a qualified conclusion that STETS represents a good social investment. If operational costs can be held at the level observed when the demonstration was operating relatively smoothly (i.e., during the five-month "steady-state" period), then the benefits generated by STETS can be assumed to outweigh its costs, as long as the 22-month impacts on experimentals persist for at least seven months beyond the observation period. These benefits accrue primarily to program participants, who gain from their increased earnings (both while in STETS and in subsequent employment). Although the rest of society also receives substantial benefits, it must pay the taxes that fund the STETS services. Consequently, they incur a net financial cost from STETS in the short-run, although this cost may be offset by savings from long-run reductions in the use of sheltered workshops by participants, and by the desirability of increasing the opportunities available to and enhancing the social integration of mentally retarded young adults.

We begin our analysis by providing an overview of the benefit-cost accounting framework and the general analytic methods. We then present the estimated costs of the program, and discuss the value of changes in (1) the output produced by experimentals both in and out of STETS, (2) their use of sheltered workshops, school and training programs, and residential programs, and (3) their dependence on public financial assistance. We aggregate these individual measured costs and benefits in the framework and analyze them and their relation to unmeasured effects in order to assess STETS overall as an investment. A separate technical appendix (Appendix C) provides additional details on the estimates, procedures, and findings of this benefit-cost analysis.

A. BENEFIT-COST ANALYSIS FRAMEWORK AND METHODS

While countless questions can be asked about a program such as STETS, benefit-cost analysis is appropriate for addressing only two: Was the program economically efficient? And was it equitable? Efficiency pertains to the effect of a program on the total value of the goods and services available to society (i.e., was the value of the goods and services available to society greater because of STETS, or would their value have been greater if the resources that were devoted to STETS were used for alternative purposes?). Equity pertains to the distribution of goods and services among groups in society, how STETS affects that distribution, and whether a specific group of individuals benefits or loses.

In general, the methods used in benefit-cost analysis are oriented toward addressing efficiency: program efficiency can usually be determined, and it is presumed that greater program efficiency is desirable. Equity questions are more difficult to answer. Benefit-cost analysis can (to some extent) determine the effect of a program on the distribution of resources, but contains no special criteria for judging whether a distributional change is desirable. Thus, addressing equity entails a more descriptive analysis, and conclusions must be based on a broader analysis of public policy and social concerns.

The basic method used to determine economic efficiency is to assign dollar values to all estimated impacts and costs. These values are then summed together to yield an estimate of the program's net present value--that is, the difference between benefits and costs, where all dollar values are adjusted to reflect their value in a specific base period. A positive net present value indicates that the resources are being used efficiently.

While the net present value criterion can easily be stated, a high degree of uncertainty surrounds its estimation: program effects are measured imperfectly, and some cannot be estimated at all; uncertainties surround the values that should be placed on the specific program effects or costs; and the appropriate techniques necessary to aggregate individual benefits and costs inherently involve numerous approximations. Consequently, it is difficult to apply the net present value criterion to judge the economic efficiency of the program.

Because of the error associated with any single estimate of net present value, much of the usefulness of benefit-cost analysis pertains to its comprehensiveness. The process of drawing together measures of the various inputs and outcomes and the general patterns that emerge from the attempts to assign relative values are often more useful than any specific estimate of net present value. For this reason, the analysis does not focus on a single net present value estimate but, instead, on a set of estimates. By examining the different assumptions, the underlying outcome estimates, and the techniques used to value outcomes, it is possible to identify those aspects of STETS and its evaluation that are most important in determining the overall findings--that is, which (if any) aspects, if changed, would change the basic nature of the findings.

The core of this approach is a benefit-cost accounting framework that imposes a logical rigor on the analysis and serves as a guide for interpreting the results. The framework specifies a consistent method for valuing the diverse sets of effects. The approach used here is based on concepts similar to those that underlie the estimation of gross national product (GNP). It focuses on the net resource gain or loss induced by STETS as it was implemented in the demonstration. Essentially, the approach entails estimating the change in resources available because of STETS, and then valuing those resources at their market cost. Thus, for example, STETS-induced reductions in the use of sheltered workshops by experimentals enables society to reallocate some of the resources that would have been used to operate those programs.[1] The market value of the saved resources is used as a measure of the value generated by the reduction in sheltered workshop use. In general, this valuation procedure is convenient to use and does not necessitate attempting to measure such difficult concepts as the willingness of society to pay for the various outcomes.

While this procedure assigns a value to program effects, that value will be viewed differently by different groups. For example, experimentals who lose SSI benefits after obtaining a job will view the loss as a cost, while taxpayers will view it as a financial gain. The accounting framework captures these differences through three analytical perspectives: society as a whole, program participants, and nonparticipants.[2] The perspective of society as a whole abstracts from all of the redistributional aspects of STETS and focuses on its efficiency, since it considers only the use of resources. Transfers of income between groups are assumed to cancel each other out in the social perspective--that is, a dollar of benefit or cost to one person is assumed to be equivalent to a dollar of benefit or cost to anyone else.[1] The perspectives of participants and nonparticipants facilitate an analysis of the distributional consequences. For each group, the question for net present value is the same as it was for society as a whole: Does net present value (from that perspective) exceed zero? Do participants gain or lose, on average, from their participation? Are their earnings gains and increased independence sufficient to outweigh the losses of transfer benefits such as SSI? How are nonparticipants affected? Does STETS require a net subsidy from nonparticipant taxpayers?[2]

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[1] Of course, one possible reallocation would be to continue to operate the sheltered workshops at the same scale and to serve clients who would not have been served in the absence of STETS.

[2] The nonparticipant group includes everyone in society who is not given the opportunity to participate in STETS. Thus, it encompasses much more than the control group, which comprises a very small part of the nonparticipant group. We prefer this term to the more common term "taxpayer group," because the participant group also pays taxes, and because not all of the effects on nonparticipants occur through the tax system.

All three perspectives mask differential effects on specific individuals or groups. The impacts of STETS are measured as averages and indicate the expected effect of STETS. Obviously, participants will differ in their response to STETS and may do better or worse than the statistical averages. In addition, individual nonparticipants will be affected differently. The employer who is able to hire a productive worker because of STETS will perceive the program differently than will an average taxpayer who helps to fund STETS. Therefore, the STETS benefit-cost estimates must be taken as indicative of the expected overall effects of STETS, viewed from a broad perspective that is appropriate for judging aggregate program performance. A more detailed analysis would be required to answer questions about whether specific types of individuals would benefit from enrolling in STETS.[1]

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[1] Of course, any resources consumed in transferring income would be counted as costs from the social perspective.

[2] One analytically useful feature of using these three perspectives is that the sum of the net present values calculated from the participant and nonparticipant perspectives equals the net present value for the social perspective. This "adding-up" property is valid because participants and nonparticipants constitute mutually exclusive groups that, when combined, include all members of society. Therefore, transfers of income between these two groups cancel each other out in the social perspective, because the benefit to one group is assumed to be equal to the cost to the other. Benefits or costs that accrue to one group and that are not offset by corresponding costs or benefits to the other (e.g., increased work output) do not cancel out when added, and they thus represent net social benefits or costs.

Because the benefits and costs of STETS occur over time, the analysis must compare streams of benefits with streams of costs. To simplify this task, we include in the accounting framework several procedures for aggregating dollars in different time periods and for producing equivalent estimates of benefits and costs at a single point in time. To do so, it is necessary to account for differences in the value of benefits and costs across time periods due to inflation and to foregone interest earnings. The inflation differences are corrected by valuing all benefits and costs in 1982 dollars. Thus, differences between benefits and costs reflect real changes in resources, not changes in the nominal value of a dollar. The differences due to foregone interest reflect the fact that a benefit that occurs in the future is worth less than the same benefit that occurs today, because today's savings could be invested and would earn interest in the future.[2] The procedure for adjusting for such differences is called "discounting," and its importance is well established among the analytic literature (see, for example, Gramlich, 1981). The only uncertainty that remains pertains to the interest rate that should be used in discounting future benefits and costs. We assume a 5 percent real annual rate (that is, a rate calculated by netting out inflation) for our benchmark, and test the importance of this assumption by calculating alternative estimates using real annual rates of 3 and 10 percent.[3]

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[1] The subgroup analyses in Chapters IV through VII provide some information on the impacts of the program on individual types of participants.

[2] For example, suppose that a $1,000 benefit occurs 10 years from now. The issue would then be, what present value invested today would yield $1,000 ten years from now? If interest rates were, for example, 5 percent, then this present value would equal $1,000 divided by $(1 + .05)^{10}$, or $614. Gramlich (1981) describes this process in more detail.

[3] The Office of Management and Budget (1972) mandates a 10 percent discount rate for evaluating government programs.

(Appendix C provides further information on the rationale and procedures for discounting.)

Table VIII.1 presents the benefit-cost accounting framework. The table lists the major impact components of STETS (regardless of whether we can value them) and suggests whether a component is, on average, a benefit, a cost, or neither from each of the three perspectives.[1] The table also indicates what data sources are used to measure and value the effects or whether a particular effect is left unmeasured. The next two sections discuss the separate cost and benefit components.

Before proceeding, however, it is important that we review how the impact estimates presented in Chapters IV to VII are used in the benefit-cost analysis. These estimates indicate the effect of STETS on experimentals at 6, 15, and 22 months after randomization. These "point-in-time" estimates are adequate measures of the impacts of STETS, but are inadequate for the benefit-cost analysis, which requires information on the impacts of the program for the entire 22 months. In order to compare benefits and costs, we need to estimate the cumulative change in earnings, program use, transfer receipt, and other activities. In the absence of continuous data on these activities, it is necessary to derive cumulative measures by interpolating between the point-in-time estimates.

Any interpolation method involves some arbitrariness. We have chosen to interpolate linearly between the point estimates. This method is straightforward, and appears reasonable in that no alternative is clearly preferable. Thus, although all program impacts used in the benefit-cost analysis are inherently more imprecise than the specific impact estimates presented earlier, we feel that estimates of cumulative effects based on linear interpolations provide an accurate indication of the true magnitude

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[1] Whether an impact component will be a net benefit or cost is sometimes problematic. Table VIII.1 reflects prior judgments about the value of components from the three perspectives. The treatment of all components in the final net present value calculations is of course determined by the estimated actual effects of STETS.

TABLE VIII.1

EXPECTED BENEFITS AND COSTS OF STETS BY ANALYTICAL PERSPECTIVE

Component	Analytical Perspective			Data Source[a]
	Social	Participant	Nonparticipant	
I. Program Costs				
Project operations	-	0	-	A
Payments to participants	0	+	-	A
Central administration	-	0	-	P
II. Output Produced by Participants				
Phase 1 and Phase 2 output	+	0	+	S
Output forgone while in STETS	-	-	0	I,P
Increased out-of-program output	+	+	0	I,P
III. Other Programs				
Reduced use of:				
Sheltered workshops	+	0	+	I,P
Work-activity centers	+	0	+	I,P
School	+	0	+	I,P
Job-training programs	+	0	+	I,P
Case-management services	+	0	+	E
Counseling services	+	0	+	E
Social/recreational services	+	0	+	E
Transportation services	+	0	+	E
IV. Residential Situation				
Reduced use of:				
Institutions	+	0	+	I,P
Group homes	+	0	+	I,P
Foster homes	+	0	+	I,P
Semi-independent residential programs	+	0	+	I,P
V. Transfer Payments and Taxes				
Reduced SSI/SSDI	0	-	+	I,P
Reduced other welfare	0	-	+	I,P
Reduced Medicaid/Medicare	0	-	+	I,P
Increased taxes	0	-	+	I,P
VI. Transfer Administration				
Reduced use of SSI/SSDI	+	0	+	I,P
Reduced use of other welfare	+	0	+	I,P
Reduced use of Medicaid/Medicare	+	0	+	I,P
VII. Intangibles				
Preferences for work	+	+	+	U
Increased self-sufficiency	+	+	+	U
Increased variation in participant income	-	-	-	U
Foregone nonmarket activity	-	-	0	U
Increased independent living	+	+	+	U

NOTE: The individual components are characterized from the three perspectives as being a net benefit (+), a net cost (-), or neither (0).

[a] The codes used for the data sources are as follows: S-special study, I-interview data, P-published data source, A-STETS accounting system data, U-item not measured, and E-item measured but excluded because the effects of STETS were trivial.

of program impacts.[1] (Details on the interpolation procedures are provided in Appendix C.)

B. STETS PROGRAM COSTS

The accounting framework disaggregates costs into three components: the operating costs of the projects, compensation paid to clients while they were in Phase 1 or Phase 2 activities, and central administrative costs. The operating and central administrative costs are paid by nonparticipants. Because these costs represent the value of the resources used to operate STETS, they also represent social costs. Participant compensation is treated as a transfer from nonparticipants to participants, because it represents a shift in resources from one group to another.[2]

1. Operating Costs

During the 27 months of operations, the five projects served 284 clients and reported operating costs of almost $2,500,000, implying average operating costs of $8,800 per client. However, for two reasons, this estimate is misleading for the benefit-cost analysis. First, it corresponds to all clients, and not to the group of 226 participants who were included in the research sample. Second, it includes costs that are attributable to the fact that STETS was a demonstration.

The first problem can be corrected in part by adjusting for differences between experimentals and other clients in their length of program participation. We estimated the cost of serving an active client for a month and then multiplied this average cost by the observed average length of time during which experimentals were active in the demonstration. This method is accurate as long as the five demonstration projects provided the same level of service per month to experimentals as they did to clients who were not in the research sample. Because there is no evidence of such differences in service provision, we feel that the method is sound.

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[1] Data on program costs and participation in STETS were obtained from demonstration accounting and client-tracking records. Because these records provided data for the full observation period, interpolation between point estimates was unnecessary for estimating the following: STETS program costs, STETS payments to participants, and the value of output produced by participants while in Phase 1 and Phase 2 jobs.

[2] The output produced in Phase 1 and Phase 2, which is related to this compensation, represents real resource gains. Section C discusses both this output and changes in output produced outside of STETS.

It is more difficult to correct for the special demonstration costs. Our general rule was to include all costs of the demonstration as it was fielded, with two exceptions. First, we subtracted an estimate of research-related costs--the costs of finding and screening applicants who were ultimately assigned to the control group, of completing and processing the research data collection forms, and of staff time spent with the researchers. Riccio and Price (1984) estimated that these costs constituted 5 percent of total project expenditures (including both operating and participant compensation expenditures). Because these research costs probably had little or no effect on the impacts of the program, we feel that they should be excluded from the benefit-cost analysis.

We made the second exception to the principle of estimating operating costs as actually incurred in the demonstration because several demonstration-specific features made the observed costs abnormally high. In particular, the limited duration of the demonstration meant that the actual costs overrepresented the higher average costs of the initial project implementation and of the demonstration phase-down period. Additional costs were also incurred because projects found it necessary to take special precautions to deal with the funding uncertainties surrounding the demonstration itself. Riccio and Price (1984) discuss these problems and suggest that the costs incurred during a five-month "steady-state" period best represent the costs of operating STETS on an ongoing basis. This period (which covers slightly different months at each site) fell in mid- to late-1982, a period when enrollments were high and operations were stable relative to earlier and later periods. We have used average costs from these periods in the benefit-cost analysis.[1]

After making these two adjustments for research costs and the effects of start-up and budgetary uncertainties, we estimate that it costs $666 per month of active participation to provide STETS services.[2] According to data collected by MDRC as part of its client-tracking system, experimentals were active for an average of 9.3 months. When discounted at a 5 percent real annual rate to the point of enrollment in STETS, the implied participation cost is $6,050 per participant.

2. Participant Compensation

Participant compensation includes the wages and fringe benefits that the five projects paid to participants in Phase 1 and Phase 2 activities. It also includes wages paid by employers directly to participants in Phase 1 and Phase 2. The projects reported all these expenditures monthly to MDRC as part of the demonstration monitoring and accounting system. During the five months of the steady-state period, participant compensation averaged $341 per month of active participation (average compensation expenditures over the entire demonstration were $30 lower than during the steady-state period). Given the estimated average length of active participation (9.3 months), this figure implies that participants received Phase 1 and Phase 2 compensation worth $3,094 per participant when discounted to the time of enrollment.

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[1] As mentioned in Chapter IV, employment impacts were greatest for persons who were served during the steady-state period, suggesting that the higher average costs during the demonstration start-up and phase-down periods occurred because the projects were establishing new procedures and dealing with inefficiencies due to small and changing scales, rather than because extra services were being provided. This finding further supports the use of steady-state costs in the benefit-cost analysis.

[2] Riccio and Price (1984) reported costs on the basis of enrollment months rather than active months. Because some clients were inactive for part of their enrollment period, the cost per enrollment month will be lower than the cost per active month. However, the average length of participation will be correspondingly higher when measured in enrollment time rather than in active time. Thus, the cost per client (the product of the cost per month and the months of participation) is independent of how time is measured. (Appendix C discusses the cost-estimating procedures in greater detail.)

3. Central Administrative Costs

Central administrative costs cover the activities necessary to administer the contracts with the five projects and to provide demonstration-wide coordination. MDRC performed this task in the demonstration, although state or federal agencies would probably assume this role in an ongoing program. For example, a state vocational rehabilitation agency could fund the programs and would assume responsibility for audit and performance monitoring. Estimating central administrative costs was difficult in STETS because of the overlap between MDRC's monitoring and research activities. Their dual role in the demonstration meant that their costs exceeded the central administrative costs that would be incurred in an on-going program. In addition, they incurred substantial start-up costs as they selected the sites and helped them operationalize the program. Consequently, the demonstration experience does not provide an adequate guide to estimating future central administrative costs. Our benchmark estimate is that central administrative costs would be approximately $20 per month if STETS were operated in a fairly decentralized manner whereby most of the monitoring would focus on audit responsibilities and fairly straightforward performance measures. Costs would be higher if the central authority provided intensive monitoring or technical assistance.

C. OUTPUT PRODUCED BY PARTICIPANTS

The analysis of STETS-induced effects on output produced by participants distinguishes between goods and services produced by participants in Phase 1 and Phase 2 and those produced by them outside of STETS. These two types of output have different distributional consequences and necessitate using different estimation techniques.

1. Value of In-Program Output

The value of output produced by participants in Phase 1 and Phase 2 is an important program benefit. This output accrues to nonparticipants (and to society) and has been very important in previous benefit-cost studies.[1] One measure of the value of in-program output is the revenue generated either by payments made by firms which used participant labor or from the sale of participant-produced goods. This valuation method provides a reasonable lower-bound estimate of the value of in-program output, because the output was actually purchased for this amount.[2] However, the STETS sites did not pursue revenue-generating strategies as their primary goal; rather, they focused on securing placement and training for the STETS participants. Thus, revenue may seriously underestimate the actual value of output. Based on estimates by Riccio and Price (1984), revenue for the five-month steady-state period was over $108,000, or $131 per active participant month. Thus, revenue offset almost 40 percent of participant compensation.

As an alternative method for estimating the value of the in-program output of participants, we conducted a series of work-activity case studies for 33 randomly selected experimentals.[3] For each person, we estimated the net value of the output they produced during a two-week reference period.[4] This estimate was based on the alternative supplier's price of the participant output--that is, on the wages and fringe benefits that would have been paid by an employer to other workers to produce the output that was produced by the participant. This estimate assumes that employers can obtain additional labor at the wages paid to their regular employees. To obtain the net value added by participants, we subtracted from the alternative supplier's price an estimate of the costs of additional employer-provided supervision and reduced output from other workers from using the participant labor.

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[1] For example, in the national Supported Work demonstration, the value of output offset approximately 65 percent of the social costs (see Kemper and Long, 1981, p. 269).

[2] Revenue is a lower-bound estimate under the assumption that profit-seeking employers would not pay more than a product (participant labor service) was worth to them. Thus, the value of output should not be less than what was actually paid. Of course, this argument is weakened if altruism prompts employers to overpay because of their desire to support the STETS program and its participants. In either event, the direct estimates of the value of output (discussed in the text) offer a more accurate estimate of the resource value of participant output.

[3] We also studied seven participants who were not included in the research sample. The results for these participants were similar to the results for the 33 experimentals (see Appendix C).

[4] The studies were completed between September and December 1982, the period that is generally considered to be part of the "steady-state" period.

These estimates of the net output are based on in-person interviews with the participants' supervisors, and rely on those supervisors' judgments about participant productivity, supervisory costs, the reactions of other staff, and the source and cost of alternative labor to perform the participants' tasks. These estimates are also subject to problems because of the difficulty in assessing whether the use of STETS-participant labor enabled the employer to increase output. There were several instances where an employer took on a STETS participant and raised the quality of the output, rather than increasing the amount or price of that output. For example, one STETS participant was hired by a day-care program operator. The operator did not change prices or increase enrollments, but, instead, apparently used the STETS participant to increase the amount of attention given to the children in the day-care program. Our net value-of-output estimates do not adjust for such quality changes. Thus, our estimates may be too low in some cases in which quality changes occurred.

Despite these limitations, the estimates of the net value of participant output represent useful indicies of the value of in-program output. They are based on careful, systematic case studies, and, where possible, incorporate actual wage, fringe-benefit, and production records in their derivation. The net-value-added estimates indicate that participants in the research sample produced output worth an average of $293 per month of active participation during Phase 1 and $503 per month during Phase 2. Given an estimated average length of active participation of 5.5 months in Phase 1 and 3.8 months in Phase 2, these figures imply a total value of output, discounted to the time of enrollment, of $3,434 per participant (an amount that would more than offset participants' in-program compensation).

2. The Value of Out-of-Program Output

STETS affected output produced outside of the program in two ways. As participants entered STETS, they gave up some alternative employment opportunities. Later, as they completed their STETS training, many participants were able to work more than would have been the case in the absence of STETS. These changes in output enter the benefit-cost analysis from the perspectives of society and participants.

Participants will perceive foregone production as a cost and increases in production as benefits. These changes also enter the social perspective to the extent that they represent a net change in total output. It is generally assumed that this is the case. However, if participants displaced workers who would have otherwise held the jobs filled by participants, then the lost output of those other workers must be subtracted from the increased output of participants in order to calculate the net change in social resources. In the extreme, STETS may have simply shuffled workers among a fixed number of jobs, with no net increase in output. The participants would have had higher incomes, but at the expense of other workers who were displaced.

At the other extreme, STETS may have enabled participants to move from a labor market in which an excess supply of labor existed to one in which an excess of demand existed. When participants leave the market in which an excess of supply exists, any jobs that they would have obtained are filled by workers who would have been unemployed otherwise. From the social perspective, this effect implies that no output is foregone by having participants enter STETS, and thus that the social cost of participation is zero. In this case, social benefits would equal the increase in participant earnings plus the increased earnings of nonparticipants who fill jobs vacated by participants. Of course, this result requires that participants not be placed back into the excess labor-supply market.

STETS jobs often seemed to be in markets in which an excess supply of labor existed. The demonstration was fielded during an economic recession, and the possibility of displacement seems relatively high, both on the jobs that they held and on those that they would have held. These indirect labor-market effects will affect the social value of the output produced in Phase 1 and Phase 2, as well as the social value of the output produced after leaving STETS.

The net change in social output in the presence of displacement can be valued in several ways. We chose to estimate net present value under the assumption that no displacement occurred, and then to assess the importance of this assumption by calculating alternative estimates under the assumption of some displacement. This approach can be thought of as indicating the potential of STETS to increase social output, provided that macroeconomic conditions are adjusted to take advantage of it. Moreover, given the absence of any empirical basis for estimating the extent of indirect labor-market effects (or even knowing the direction of their net effect), no clearly preferable alternative exists.

Under these assumptions about indirect labor-market effects, the change in out-of-STETS output was estimated as the change in total compensation received by participants (i.e., gross wages plus fringe benefits). If the markets function competitively, then the actions of employers and workers will ensure that total compensation equals the value of workers' contribution to output. In addition, regulations pertaining to wage rates in sheltered workshops require that compensation in that sector reflect productivity. To estimate total compensation, we multiplied the after-tax earnings estimates derived from interview data by a factor that reflected tax-withholding and fringe-benefit rates. This factor indicates that total compensation is 45 percent larger than after-tax earnings.[1] To ensure that the estimates correspond to out-of-program output, we used non-STETS earnings as our measure.

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[1] As described in Appendix C, gross earnings (i.e., before tax withholding) are 23 percent greater than after-tax earnings. In addition, fringe benefits for low-wage workers such as STETS participants were estimated to be 18 percent of gross earnings.

The resulting calculations indicate that at the 6-month point, when most experimentals were in STETS, the controls had $16.89 more per week in non-STETS total compensation than did experimentals. This differential was reversed at month 15, when experimentals earned $5.76 more total compensation per week than did controls. At month 22, when all experimentals had left STETS and all earnings were from non-STETS employment, the experimental-control differential in total non-STETS compensation had risen to $13.50 per week.

These estimates, along with the interpolations, are shown in Figure VIII.1 (a zero experimental-control difference was assumed for the time of enrollment).[1] The cumulative change in total compensation--which equals the value of out-of-program output--is shown as the shaded area. It indicates that, during the fifteen months after randomization, participants forewent non-STETS jobs in which they would have produced output worth $437 per participant. During the seven months between month 15 and month 22, participants produced increased non-STETS output worth $290 per participant. When discounted to the time of enrollment, these estimates imply $425 of output foregone per participant in the first fifteen months and a subsequent increase in output worth $268 per participant.

We have used these two figures to approximate both the foregone output while participants were in STETS and the increased postprogram output. Of course, since most participants spent less than fifteen months in STETS, this approximation is fairly rough. However, despite the imprecision in this disaggregation, the sum of the two estimates is an accurate estimate of the net change in non-STETS output during the 22-month observation period.

D. OTHER BENEFITS OF STETS

While the primary objective of STETS was to increase employment and earnings, the intervention also generated other important impacts. We

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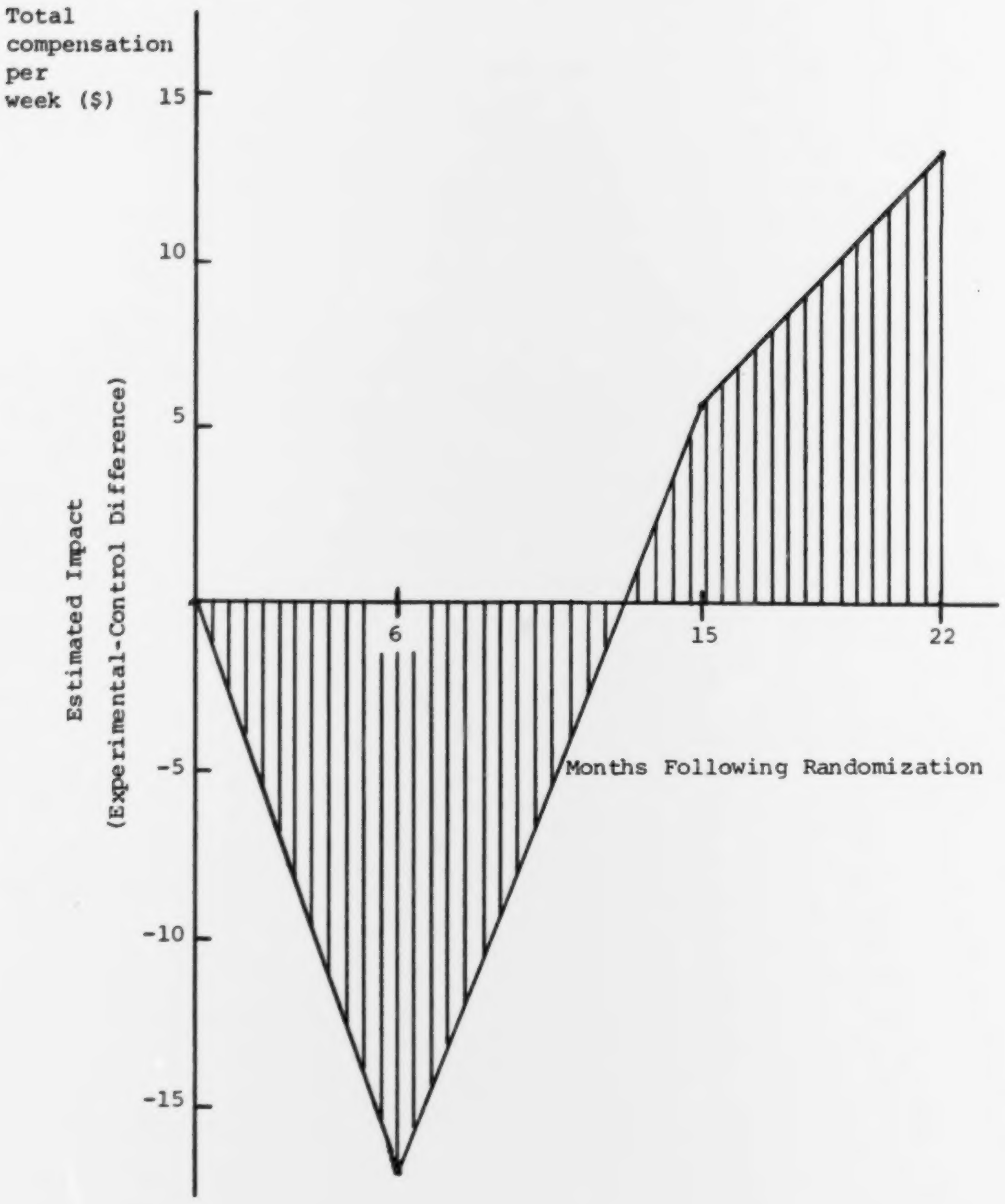
[1] Details on the calculation of total compensation and the interpolation are provided in Appendix C.

FIGURE VIII.1

CUMULATIVE IMPACT ON TOTAL COMPENSATION PER PARTICIPANT FOR THE 22-MONTH OBSERVATION PERIOD



consider these impacts within the context of three groups: (1) changes in the use of programs other than STETS, (2) changes in the use of residential programs, and (3) changes in government transfers and taxes.

In general, these impacts were estimated by multiplying the estimated impact on months of use by a "shadow price," which is an estimate of the average monthly cost of use. For example, changes in the use of sheltered workshops by participants were valued by multiplying the estimated change in the number of months that participants used sheltered workshops by the average monthly cost of operating sheltered workshops. The changes in use were based on interpolation procedures similar to those used to estimate the change in non-STETS earnings. The monthly cost estimates were based on published data on the costs of the various programs. As mentioned in Section A, all benefits were valued in 1982 dollars to correct for the effects of inflation. Thus, the monthly cost estimates used to value changes in use reflect 1982 dollars.

1. The Use of Training and Service Programs Other than STETS

Participants were expected to alter their use of training and service programs other than STETS. As was shown in Table VIII.1, these alternative programs included sheltered workshops, work-activity centers, schooling, job-training programs, case-management services, counseling, social/recreational programs, and transportation programs. In many cases, particularly for sheltered workshops, STETS was observed to reduce the average use of these programs, but it could have increased use for some persons who needed to supplement their STETS services. Any changes in the use of these other programs consume or free up resources. Thus, such changes are social costs or benefits. These changes also enter into the nonparticipant perspective because that group funds these programs.

Table VIII.2 presents the estimated changes in the use of four types of alternative programs: (1) sheltered workshops,[1] (2) secondary-

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[1] Because few sample members actually used work-activity centers, we combined workshops and activity centers into a single category.

TABLE VIII.2

ESTIMATED VALUE OF BENEFITS PER PARTICIPANT FROM REDUCED USE OF TRAINING AND SERVICE PROGRAMS OTHER THAN STETS

Program	Estimated Change in Months of Use: Months 1-6	Months 7-15	Months 16-22	Estimated Value Per Month (1982 Dollars)	Estimated Benefit (Total Discounted Value in 1982 dollars)[a]
Sheltered Workshop	-0.19	-0.72	-0.74	493	767
Secondary Vocational Special Education	-0.20	-0.42	-0.24	520	428
Other School	-0.11	-0.25	-0.04	290	112
Other Job-Training Programs	-0.99	-2.09	-0.92	113	434
Total Benefits					$1,741

NOTE: Details on the derivation of the cumulative impact estimates and the estimates of average value per month for the programs are presented in Appendix C.

[a] Because reductions in use are classified as benefits, the discounted values are listed as positive numbers. All dollar values are expressed in 1982 dollars and are discounted to the time of enrollment using a 5 percent real annual rate.

level vocational programs, (3) other schooling, and (4) alternative job-training programs (other than schooling or sheltered workshops). We estimated reductions in the use of all these programs over the 22-month observation period, with the largest reductions observed for alternative training programs and sheltered workshops. When these changes are valued and discounted to the point of enrollment, the value of the changes in program use is estimated to be $1,741 per participant. Thus, savings from the reduced use of alternative training and service programs over the first 22 months offset approximately 30 percent of the operating costs.

2. The Use of Residential Programs

Most participants lived with their parents or relatives and continued to do so over the 22-month observation period. Thus, only small changes occurred in the use of residential programs. We estimated that slight reductions occurred in the use of institutions, group houses, and foster care; a small increase occurred in the use of semi-independent residential programs (e.g., supervised apartments), particularly at the 15- and 22-month observation points. The discounted net value of these observed changes in the use of residential programs is $149.

Our estimates of the impacts on living arrangements may fail to capture the longer-run effects of STETS. The STETS population is fairly young, and most sample members lived with their parents or relatives. This situation may not continue in the long run. Certainly, most young adults in the age range of the STETS target group would be moving into more independent living arrangements. Whether the employment and income impacts of STETS will induce participants to make such moves or prompt them to move into particular types of supported arrangements is uncertain. This potential long-run impact should be kept in mind when assessing the observed benefits and costs of STETS.

3. The Use of Government Transfer Programs and Taxes

We examined the use of three types of government transfer programs: SSI and SSDI, other cash transfers, and Medicaid and Medicare. For each program, we estimated the change in benefit payments (which represent transfers between nonparticipants and participants) and the changes in program administrative costs (which represent the social and nonparticipant resource costs associated with making the transfers). Changes in benefit payments for SSI/SSDI and other transfer programs were estimated directly from interview data (interview self-reports were adjusted to reflect 1982 dollars). Changes in Medicaid/Medicare benefits were based on estimated changes in coverage (based on interview data) and on the average annual expenditure per disabled Medicaid recipient. Administrative costs for all programs were based on changes in transfer-program use and on published data on average administrative costs.

As shown in Table VIII.3, the largest savings were generated from reductions in SSI and SSDI. Medicaid/Medicare savings were also substantial. Together, the net savings from reduced transfer payments were estimated to be worth $545 per participant (after discounting). The corresponding administrative cost savings were estimated to be $31 per participant.

Tax payments by participants are also viewed as transfers. An increase in tax payments is a cost to participants, but represents an offsetting benefit to nonparticipants (i.e., all other taxpayers); hence, they are transfers and do not enter into the social perspective.[1]

To estimate paid taxes, we used an estimate of the change in participants' total income and an estimate of the overall tax rate applicable to low-income households. As estimated by Pechman (1985), this tax rate was approximately 23 percent of total income, defined to include earnings, public transfers, training allowances, etc. The major components of this tax rate are payroll, sales, and excise taxes. These taxes are difficult to avoid, especially those levied on consumption. Thus, even though participants generally faced low tax rates on earnings and may in

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[1] As is the case with all transfers, changes in the resource costs of making the transfers should be included in the social perspective. However, in terms of tax payments, the change in administrative costs is probably very small and is treated as zero. Moreover, this treatment of taxes ignores the additional benefits accruing to participants because their tax payments support governmental activity.

TABLE VIII.3

ESTIMATED VALUE OF BENEFITS PER PARTICIPANT FROM REDUCED USE OF TRANSFER PROGRAMS

Program	Estimated Changes[a] Months 1-6	Months 7-15	Months 16-22	Estimated Value Per Month (1982 Dollars)	Estimated Benefit (Total Discounted Value in 1982 Dollars)[b]
Transfer Payments					
SSI/SSDI	-31.30	-125.80	-121.9	1	264
Other Welfare and Cash Transfers	-23.90	-68.10	7.8	1	82
Medicaid/Medicare	-0.19	-0.52	-0.27	215	200
					545
Administrative Costs					
SSI/SSDI	-0.14	-0.55	-0.45	13	14
Other Welfare and Cash Transfers	-0.25	-0.48	0.08	11	7
Medicaid/Medicare	-0.19	-0.52	-0.27	11	10
					31
Total Benefits					576

NOTE: Details on the derivation of the cumulative impact estimates and the estimates of average value per month for the programs are presented in Appendix C. Details do not always sum to totals because of rounding.

[a] For transfer payments from SSI/SSDI and other welfare, the charges are in dollars of payments received. For administrative costs for SSI/SSDI and other welfare, charges are in months of use. Changes in Medicaid/Medicare are in months of coverage.

[b] Because reductions in use are classified as benefits, the discounted values are listed as positive numbers. All dollar values are expressed in 1982 dollars and are discounted to the time of enrollment using a 5 percent real annual rate.

fact have avoided paying some payroll and income taxes, their total tax burden (as a percentage of income) was not significantly different from the tax burden of most taxpayers (although the composition of taxes did vary considerably by income level).

The change in total income was calculated from estimates of the changes in gross earnings and transfer payments, which is essentially the same measure of income used in Chapter VII, with the addition of an imputed value of tax withholding to change from after-tax to gross earnings. Altogether, we estimated that participants paid an average of approximately $250 more in taxes during the 22-month observation period.

E. OVERALL ASSESSMENT OF BENEFITS AND COSTS

In assessing STETS as an investment, we first aggregated the measured benefits and costs described in the previous sections. We then considered the intangible (unmeasured) benefits and costs pertaining to changes in employment, social integration, and independence. Thus, the final assessment of STETS reflects both the measured and unmeasured impacts and costs.

1. Measured Benefits and Costs

The individually measured benefits and costs are added together in Table VIII.4. The estimates suggest that STETS created a net cost to society during the 22-month observation period. The measured social costs totaled $6,232 per participant, while measured social benefits (increased output produced by participants and the reduced use of other training, service, residential, and transfer programs) totaled only $5,199 per participant. Participants clearly benefitted from their participation, receiving in-program compensation that more than offset their tax payments and their reduced use of transfers. Nonparticipant taxpayers incurred the costs both for operating STETS and for participant compensation. They received substantial benefits (primarily from the increased output produced by participants in STETS and the reductions in their use of sheltered workshops, other job-training programs, and transfer programs), but these benefits offset only two-thirds of the costs incurred by nonparticipants.

TABLE VIII.4

ESTIMATED BENEFITS AND COSTS OF STETS PER PARTICIPANT DURING THE OBSERVATION PERIOD, BASIC ESTIMATES
(1982 dollars)

	Analytical Perspective		
Component	Social	Participant	Nonparticipant
I. Program Costs			
Project operations	-$6,050	$0	-$6,050
Payments to participants	0	3,094	-3,094
Central administration	-182	0	-182
II. Output Produced by Participants			
Phase 1 and Phase 2 output	3,434	0	3,434
Foregone output while in STETS	-425	-425	0
Increased out-of-program output	268	268	0
III. Other Programs			
Reduced use of:			
Sheltered workshops	767	0	767
Secondary vocational school	428	0	428
Other school	112	0	112
Job-training programs	434	0	434
IV. Residential Programs			
Reduced use of:			
Institutions	174	0	174
Group homes	72	0	72
Foster homes	7	0	7
Semi-independent residential programs	-114	0	-114
V. Transfer Payments and Taxes			
Reduced SSI/SSDI	0	-264	264
Reduced other welfare	0	-82	82
Reduced Medicaid/Medicare	0	-232	232
Increased taxes	0	-249	249
VI. Transfer Administration			
Reduced use of SSI/SSDI	16	0	16
Reduced use of other welfare	8	0	8
Reduced use of Medicaid/Medicare	12	0	12
VII. Intangibles			
Preferences for work	+	+	+
Increased self-sufficiency	+	+	+
Increased variation in participant income	-	-	-
Foregone nonmarket activity	-	-	-
Increased independent living	+	+	+
Net Present Value (Benefits less Costs)	-$1,038	$2,111	-$3,149

NOTE: Benefits and costs are discounted to the time of enrollment using a 5 percent real annual discount rate.

Although costs exceeded benefits for the observation period, trends in the impacts on earnings and the use of sheltered workshops and other job-training programs suggest that benefits will outpace costs beyond the observation period. Twenty-two months after randomization, measured benefits appeared to be exceeding costs at an annualized rate of almost $1,800 per participant (when discounted to the time of enrollment). Thus, as shown in Table VIII.5, measured social benefits will exceed measured social costs if the benefits observed at month 22 continue undiminished for as little as seven months. This pattern of results reflects the investment nature of STETS. The intensive training and services are provided in the expectation that long-term impacts will be generated. Thus, almost all the costs are incurred up front, while the benefits accrue gradually over time. This pattern of benefits and costs was also observed in the national Supported Work demonstration for the AFDC target group (Hollister, Kemper, and Maynard, 1984) and in the Job Corps program (Mallar et al., 1982)--two training programs that were judged successful from a social benefit-cost perspective.

Of course, this result depends on participants' maintaining their regular jobs. The stability of job-holding for sample members who had jobs appeared to be high between the 15- and 22-month interviews. Thus, it is reasonable to assume that the increases in earnings and the reductions in the use of other programs will persist for some time. However, it is unknown whether the participants will be able to maintain their employment in the long run. If they fail to maintain their jobs, then the benefit-cost assessment could change. For example, if participants' gains in earnings and regular job-holding decay linearly to the levels for controls over the year following the observation period, then social benefits will fall short of social costs.[1] Therefore, it is essential that the increased levels of regular job-holding and earnings be maintained in at least the short-run if STETS is to be considered a socially efficient investment.

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[1] If linear decay rates are assumed, then the experimental-control differentials in earnings and the use of other programs must not fall to zero until at least 15 months after the observation period if social benefits are to exceed social costs.

TABLE VIII.5

ESTIMATED SOCIAL BENEFITS AND COSTS PER PARTICIPANT IF OBSERVED IMPACTS CONTINUE FOR SEVEN MONTHS (1982 DOLLARS)

	Total Benefits (Discounted to the Time of Enrollment)		
Component	Months 1-22[a]	Months 23-29[b]	Total
Program Costs	$-6,231	$0	$-6,231
Output Produced by Participants	3,277	364	3,641
Other Training Programs	1,741	572	2,313
Residential Programs	139	103	242
Administrative Costs of Transfer Programs	37	3	40
Net Present Value (Benefits less Costs)	$-1,038	$1,042	$4

[a] This period includes the demonstration observation period.

[b] Impacts are assumed to persist for this entire seven-month period at the level that was observed at the 22-month interview.

For participants, STETS is likely to continue to represent a good investment. Because participants should continue to accumulate earnings increases (compared with what would have been the case in the absence of STETS), their net benefits should increase beyond the observation period. Even if they fail to maintain their jobs and their earnings levels fall to those observed for controls, the increases while they were in STETS and during the immediate postprogram period will generate a net gain.

For nonparticipants, STETS will continue to represent a net investment in participants, unless the effects observed at the 22-month interview persist for at least 2.5 years after the observation period. If the impacts decline, then the pay-back period (the time until nonparticipant net present value equaled zero) will be even longer. While it seems possible that impacts will persist for a sufficiently long time, we have little empirical evidence to support such a scenario at this time.

The estimated values of the benefits and costs are based on several assumptions and estimates. While these underlying assumptions and estimates are plausible, they introduce unavoidable uncertainty into the benefit-cost assessment. To evaluate this uncertainty, we examined alternative benefit and cost calculations that incorporated different sets of assumptions. The general procedure was to change one underlying assumption while keeping all others the same. These alternative estimates, which are presented in Appendix Table C.5, suggest that the overall conclusions presented above will hold as long as the operating costs can be kept to the levels observed during the steady-state period, and as long as the output produced during Phase 1 and Phase 2 has a value close to the value estimated in our case studies.

The operating costs are clearly a key factor in the perspectives of society and nonparticipants. If these costs rise, STETS will appear to be less efficient from both perspectives. If we make the extreme assumption that the appropriate cost estimates should reflect the costs that were incurred during the entire demonstration rather than just those that were incurred during the steady-state period, the operating costs (discounted to the time of enrollment by using a 5 percent real annual rate) will rise by $2,500 per participant, yielding measured social costs that would exceed measured social benefits by $3,552 per participant for the observation period. Under such a circumstance, it is uncertain whether impacts will persist long enough to yield a positive net present value.

Changes in the manner in which the value of in-program output is calculated can also affect the results. In our analysis, we measured the value of in-program output by using estimates of the net supply price. These estimates were based on data collected in case studies of 33 experimentals. They reflect our estimates of the net value added by participants, and they assume that the value is indicated by the cost of having regular workers produce the output produced by participants in Phase 1 and Phase 2.[1] As an alternative, we could have used the amount actually paid for the goods and services produced by participants in Phases 1 and 2. Because the STETS projects did not actively pursue revenue-generating policies, this alternative procedure yields a lower-bound estimate of the value. Because revenue was only 35 percent of our estimate of net supply price, using this alternative measure of the value would have reduced estimated benefits for society and nonparticipants. The resulting social benefits would have fallen short of social costs by $3,280 per participant for the observation period. For nonparticipants, costs would have exceeded their benefits by almost $5,400 per participant. Again, it is uncertain whether future benefits would make up this difference between measured benefits and costs.

Strong indirect labor-market effects could also affect the assessment of STETS. If STETS participants were placed in jobs that would otherwise have been filled by other workers, then the earnings gains of participants would overstate the true increase in output. We chose to ignore these effects in most of the analysis, focusing instead on the potential of STETS to improve participant employment. Moreover, the lack of any empirical basis for estimating indirect labor-market effects inhibited an analysis.

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[1] Gross supply price equals the cost to the most likely alternative supplier of the output. Net supply price was estimated by subtracting from gross supply price an estimate of the additional training and supervisory costs imposed on the employee by the participant.

However, we can judge how our assessment would change if these indirect effects had been taken into account. As an example, we assumed that one-half of the jobs filled by participants would have been held by other workers, and that one-half of the jobs that participants forewent when they entered STETS were subsequently filled by other workers. These assumptions had little impact on the social value of out-of-program earnings for the observation period, because the net value of those earnings was small. However, they would have reduced the social value of future earnings increases by 50 percent. The indirect effects would also have reduced the social value of in-program output from $3,434 per participant to $1,717 per participant. Because these reductions would have been borne by the displaced nonparticipants, the net present value of STETS to nonparticipants as a group would have declined. In this case, nonparticipants would have perceived a net cost of STETS of approximately $4,800 per participant for the observation period. Because participants would have been unaffected by indirect labor-market effects, the social perspective, which combines participants and nonparticipants, would have changed from a net cost of $1,038 under conditions of no indirect labor-market effects to a net cost of $2,677 under the alternative assumptions. Thus, if indirect effects existed, impacts would have to persist longer into the future in order to generate social benefits that would exceed social costs. Under the assumption of our example, we estimate that the impacts observed at the 22-month interview would have to continue for two years for social benefits to outweigh costs (recall that benefits other than earnings are not affected by displacement). We conclude that, even with substantial indirect labor-market effects, STETS can be an economically efficient investment.

The qualitative conclusions of the benefit-cost analysis were not altered by changing other assumptions about costs, the value of in-program output, and the appropriate discount rate.[1] Changes in the linear interpolations underlying the estimates of cumulative effects would clearly have affected the estimates of benefits and costs. However, our overall findings should not be affected unless (1) the pattern of results was extremely different from the estimated pattern or (2) the 15- and 22-month points-in-time were unrepresentative of actual impacts. Neither of these situations seems plausible.

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[1] If we had used the OMB-mandated real annual discount rate of 10 percent, social net present value for the observation period would have been -$1,068 per participant. Net present value for participants would have been $2,060 per participant, and would have been -$3,128 per participant for nonparticipants. These numbers are less than 3 percent different from those presented by using the 5 percent real annual rate.

2. Intangibles

STETS services were intended to enhance the economic and social self-sufficiency of participants. In achieving this goal, STETS generated the measured impacts discussed in the preceding section--increases in earnings and regular job-holding, and reductions in the use of sheltered workshops and other training programs. However, STETS also generated intangible impacts by changing the quality of life for participants. The increases in earnings and the social integration of participants were expected to increase their satisfaction, as well as the satisfaction of nonparticipants who want to expand the opportunities available to mentally retarded young adults. Such intangibles cannot be measured accurately, but they are nevertheless important components of an overall assessment of benefits and costs.

The measured impacts indicated that STETS did affect the activities and opportunities that were expected to generate intangible benefits. The increased income and the increased job-holding in the regular labor market should provide participants with benefits that go beyond the measured changes in output produced. However, in our analysis, we found limited evidence of changes in such intangibles as self-sufficiency and independence. In part, such limited evidence reflects the inadequacies of our measures and the difficulty in measuring these concepts. It may also mean that self-sufficiency responds slowly to changes in opportunities, particularly for mentally retarded young adults. These persons may feel that they must maintain their jobs and their increased social interactions for a considerable time before they alter their behavior in terms of residential situation, benefactors, and financial independence. Finally, we have no measures of any overall increases in satisfaction, other than the fact that many participants appeared to remain voluntarily in their jobs.

Changes in earnings can also create intangible costs. For example, the stress of employment may create health problems. Moreover, persons who lose their eligibility for transfers such as SSI or SSDI face potentially more uncertainty about future income than had they remained in those programs. However, it is presumed that participants must value the increased earnings and interactions by more than the costs of the intangibles, because they made the choice to enter STETS, and because many continued in their jobs after leaving STETS.

F. CONCLUSIONS ABOUT STETS AS AN INVESTMENT

The benefit-cost analysis suggests that STETS can be a worthwhile social investment. From the perspective of society as a whole, it cost $6,200 per participant to provide the STETS intervention. During the 22-month observation period, this investment yielded increases in participant output and reductions in the use of other programs by participants that offset about 85 percent of this initial investment. Consequently, we observed a measured net social cost of $1,038 per participant for the period covered by our data. However, trends observed for the impacts on earnings and the use of sheltered workshops suggest that benefits will persist and are likely to outweigh costs in the long-run. If the earnings and reduced alternative program benefits continued for as little as seven months beyond the 22-month point, social benefits would exceed social costs. This finding is consistent with other successful training programs that incur substantial up-front costs for training in order to generate long-run benefits.

In the longer-run, it is possible that benefits would substantially outweigh costs. As indicated in Appendix C, we estimate that social net present value would exceed $5,200 per participant (a benefit-cost ratio of 1.8) in ten years, even if the effects diminished by 50 percent every five years. This net benefit would be split between participants and nonparticipants, so that both groups would benefit from STETS. This finding indicates the potential of STETS, but without longer follow-up results the ultimate magnitude of net benefits is uncertain.

In addition to the measured benefits and costs included in the net present value estimates, the investment in STETS creates intangible benefits by increasing the social and employment opportunities available to the participants. While evidence is limited, the fact that many participants tended to remain voluntarily on their jobs suggests that STETS enhanced the quality of their lives. Furthermore, the increased income, regular job-holding, and social interaction provided by STETS are also expected to be benefits.

Because of their increased earnings, both during and after STETS, participants benefitted substantially from STETS. On average, they received a net benefit during the 22-month observation period of $2,111 per participant, and this amount is expected to grow as they continue to work.

For nonparticipant taxpayers, STETS represented a net cost during the observation period. This group paid not only the operating costs, but also the in-program participant compensation expenditures. Approximately two-thirds of these costs were offset by output produced by participants while in STETS, by savings from the reduced use of sheltered workshops and other programs, and by savings in transfer payments (primarily SSI/SSDI). If these impacts persist at their 22-month levels, then the nonparticipant costs will be recouped entirely within four and a half years after randomization. Nonparticipants may also receive intangible benefits from STETS because it effectively achieves a widely stated public policy goal by increasing the employment opportunities and performance of mentally retarded young adults.

IX. SUMMARY AND CONCLUSIONS

The results described in the previous chapters indicate that STETS was able to improve the employment prospects for mentally retarded young adults. It appeared to increase regular job-holding and total earnings, while generating reductions in the use of sheltered workshops. It also had small overall effects on the use of transfer programs. All in all, the program appears to be a success and will represent an economically efficient social investment if costs can be kept at or below the demonstration levels, and if impacts persist for as little as one year beyond the 22-month observation period.

In this chapter, we first summarize the main evaluation findings on the impacts of the program. Then, in Section B, we review the results of the benefit-cost analysis. Finally, Section C presents some comparisons with the results of other programs and summarizes our overall policy conclusions.

A. ESTIMATED PROGRAM IMPACTS

The analysis considered the impacts of STETS on four major areas: (1) labor-market behavior, (2) training and schooling, (3) public transfer use, and (4) economic status, independence, and life-style. Key findings are summarized in Table IX.1.

1. Labor Market Behavior

The evaluation clearly indicates that a STETS-type program can be expected to improve the postprogram employment prospects of mentally retarded young adults. As shown in Table IX.1, employment in regular jobs was significantly greater for experimental group members than for control group members in the postprogram observation period--that is, at months 15 and 22. By month 22, experimentals were an average of 62 percent more likely than controls to be employed in a regular job (31 percent versus 19 percent), and the regular-job earnings gains were proportionately larger ($36 per week among experimentals versus $21 per week among all controls).

TABLE IX.1

ESTIMATED PROGRAM IMPACTS ON KEY OUTCOME MEASURES

Outcome Measures	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Employment									
Percent Employed in Regular Job[a]	11.8	10.7	1.1	26.2	16.8	9.4**	31.0	19.1	11.9**
Percent Employed in Any Paid Job	67.8	45.2	22.6**	44.8	43.6	1.2	44.7	43.7	1.0
Average Weekly Earnings in Regular Job	$ 11.81	$ 9.81	$ 2.00	$ 26.90	$ 16.31	$ 10.59**	$ 36.36	$ 20.55	$ 15.81**
Average Weekly Earnings in Any Paid Job	$ 52.39	$ 25.93	$ 26.46**	$ 37.91	$ 26.48	$ 11.43**	$ 40.79	$ 28.41	$ 12.38**
Training and Schooling									
Percent in Any Training	61.7	40.6	21.1**	20.6	28.4	-7.8*	16.6	29.1	-12.5**
Percent in Any Schooling	7.5	15.7	-8.2**	6.2	10.1	-3.9	8.0	11.4	-3.4
Income Sources									
Percent Receiving SSI or SSDI	26.3	31.0	-4.7	33.1	40.7	-7.6**	34.9	40.2	-5.3
Average Monthly Income from SSI or SSDI	$ 66.41	$ 74.59	$ -8.18	$ 91.35	$ 109.65	$ -18.30	$ 99.27	$ 120.03	$ -20.76
Percent Receiving Any Cash Transfers	31.7	43.1	-11.4**	44.5	51.5	-7.0*	49.6	52.0	-2.4
Average Monthly Income from Cash Transfers	$ 80.23	$ 99.98	$ -19.75	$ 114.78	$ 138.72	$ -23.94	$ 126.53	$ 136.08	$ -9.55
Average Weekly Personal Income[b]	$ 71.72	$ 50.94	$ 20.78**	$ 67.22	$ 59.67	$ 7.55	$ 71.59	$ 62.39	$ 9.20

NOTE: These results were estimated through ordinary least squares techniques. Estimated impacts or selected binary and truncated outcome measures were generated using probit and tobit analysis, respectively, with virtually identical results. (These results are presented in Appendix A to the report.)

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

[b] Personal income includes earnings, cash transfer benefits (AFDC, general assistance, Supplemental Security Income, and Social Security Disability Insurance), and other regular sources of income.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

Furthermore, as shown in Figure IX.1, this postprogram increase in employment in regular jobs was roughly equal to the reduction in employment in workshops and activity centers. The STETS experience tended not to affect the average postprogram incidence of holding non-workshop-training jobs. Thus, although the overall level of employment was largely unchanged, very important compositional effects occurred. Overall, average earnings from all types of employment increased by 44 percent in month 22 ($41 per week for experimentals versus $28 per week for controls).

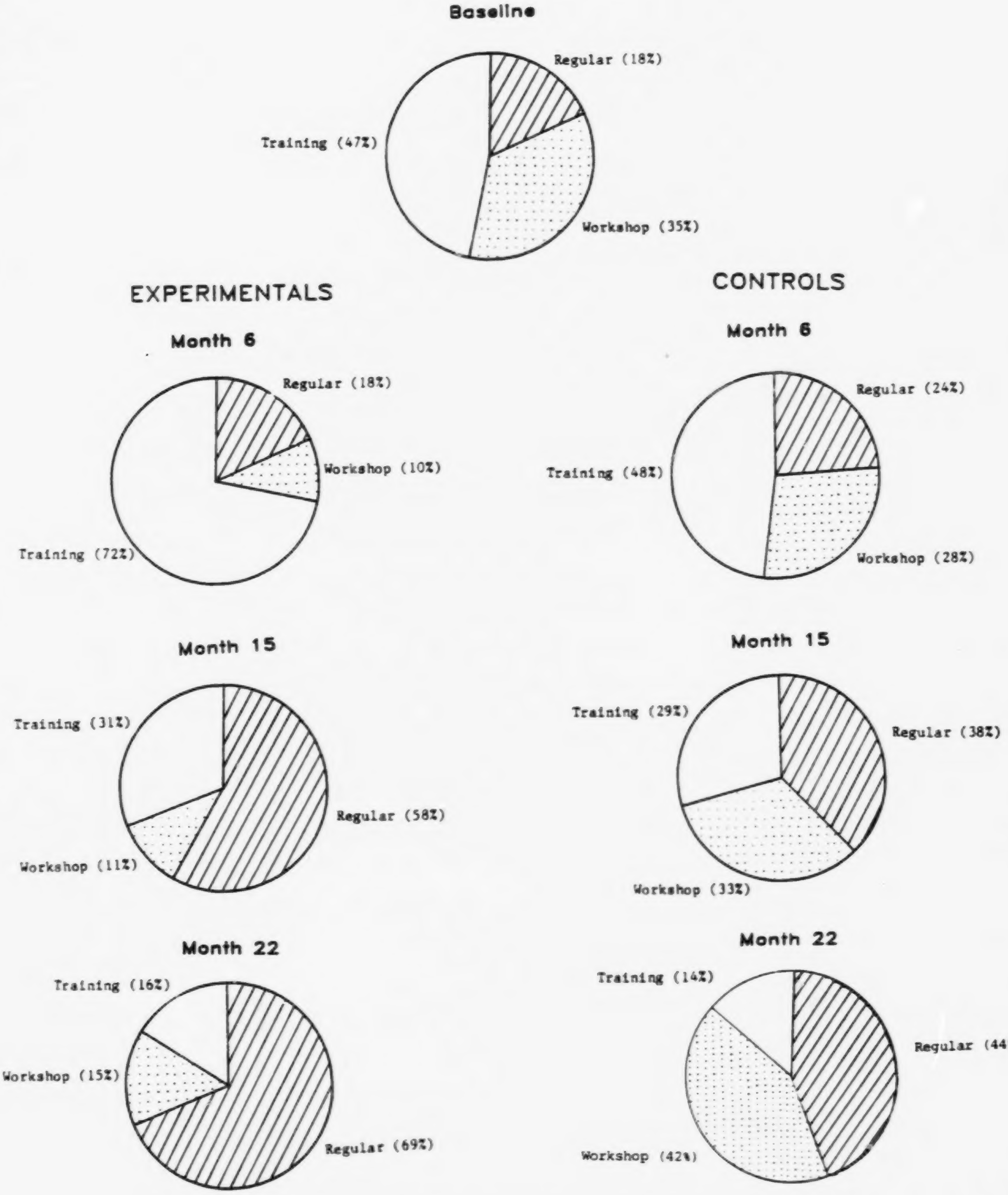
As shown in Table IX.2, the postprogram (month 22) results vary substantially among subgroups of the sample. The St. Paul program clearly had the largest impact on the regular job-holding of participants (41 percent of the experimentals, compared with 18 percent of the controls, held regular jobs). In terms of the differential impacts among subgroups defined by their personal characteristics, STETS seems to have been most effective for four groups: those with lower IQ scores and whose retardation has organic causes; older individuals; males; and those who were more independent, as evidenced by their living arrangements and money-management skills at the time they enrolled in STETS.

Of these results, two sets are especially thought-provoking--the results by IQ level and the results by gender. Estimated program impacts on the probability of holding a regular job were essentially zero for those participants with borderline IQ levels, 12 percentage points for those whose IQ scores indicated mild retardation, and 28 percentage points for those whose IQ scores indicated moderate retardation. The net effect of these differential program results is that STETS tended to raise the employment prospects for the mild and moderate retardation subgroups from levels that in the absence of STETS would have been well below those for the borderline retarded group to levels that were roughly similar to the borderline group.

The impact estimates for males and females show clear patterns which show that STETS had substantial impacts on the employment and earnings of males but no impacts on the employment and earnings of females. For example, both male and female participants would have earned about $18 to $20 per week on regular jobs ($107 per week among those employed) in the

FIGURE IX.1

TRENDS IN TYPES OF JOBS HELD BY EMPLOYED EXPERIMENTALS AND CONTROLS



NOTE: This figure is based on the regression-adjusted data presented in Table IV.4.

TABLE IX.2

ESTIMATED PROGRAM IMPACTS ON KEY OUTCOMES TWENTY-TWO MONTHS AFTER ENROLLMENT FOR KEY SUBGROUPS OF STETS PARTICIPANTS

Subgroups Defined by Characteristics at Baseline	Percent in Regular Job[a]			Average Monthly Income from SSI/SSDI			Total Weekly Personal Income		
	Experimental Group Mean	Control Group Mean	Estimated Impacts	Experimental Group Mean	Control Group Mean	Estimated Impacts	Experimental Group Mean	Control Group Mean	Estimated Impacts
Total	31.0	19.1	11.9**	$ 99.27	$ 120.03	$ -20.76	$ 71.59	$ 62.39	$ 9.20
Site									
Cincinnati	17.9	1.6	16.3	67.34	105.07	-37.73	49.84	33.77	16.07
Los Angeles	24.7	9.3	15.4	180.58	207.39	-26.81	75.76	59.66	16.10
New York	43.4	32.2	11.2	85.42	126.11	-40.69	88.96	84.59	4.37
St. Paul	41.1	17.9	23.2*	49.65	30.69	18.96	76.45	59.47	16.98
Tucson	29.6	30.8	-1.2	93.74	96.90	-3.16	65.85	68.46	-2.61
IQ Level									
Borderline	34.1	28.7	5.4	89.70	83.38	6.32	75.59	58.00	17.59
Mild	28.1	16.0	12.1**	98.02	143.75	-45.73**	66.21	67.31	-1.10
Moderate	38.2	10.7	27.5**	131.85	85.16	46.69	89.95	46.55	43.40**
Age									
Younger than 22	30.2	22.2	8.0	85.92	112.09	-26.17	68.84	60.98	7.86
22 or older	32.5	12.2	20.3**	129.21	137.86	-8.65	77.81	65.57	12.24
Gender									
Male	35.5	18.2	17.3**	91.46	133.93	-42.47**	80.41	63.46	16.95**
Female	25.1	20.3	4.8	109.26	102.24	7.02	60.23	61.00	-0.77
Race/Ethnicity									
Black	28.3	18.9	9.4	67.30	92.50	-25.20	63.34	55.79	7.55
Hispanic	48.4	25.8	22.6*	94.36	94.58	-0.22	87.20	64.85	22.35
White and other	29.7	19.2	10.5*	118.04	141.68	-23.64	72.06	65.35	6.71
Living Arrangement									
Living with parents	33.5	18.7	14.8**	102.13	111.08	-8.95	73.38	59.63	13.75**
Living in supervised setting	12.2	24.6	-12.4	78.11	102.77	-24.66	55.94	69.55	-13.61
Living independently	29.3	16.8	12.5	96.81	229.20	-132.39**	72.50	81.26	-8.76
Financial Management Skills									
Independent	41.9	23.9	18.0**	92.74	122.46	-29.72	91.76	68.78	22.98*
Not independent	26.9	17.3	9.6*	101.73	119.12	-17.39	63.90	59.95	3.95
Receipt of Transfers									
SSI/SSDI	27.7	14.5	13.2*	203.06	180.55	22.51	98.20	72.80	25.40**
Other transfers only	42.1	16.3	25.8**	53.91	114.13	-60.22**	69.86	60.15	9.71
No transfers	23.4	26.4	-3.0	36.12	63.66	-27.54	46.39	54.04	-7.65
Cause of Retardation									
Organic	33.6	9.9	23.7**	92.91	186.57	-93.66**	78.37	67.94	10.43
Nonorganic	30.4	21.0	9.4**	100.56	106.52	-5.96	70.21	61.26	8.95
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	52.3	20.9	31.4**	96.19	141.25	-45.06	84.84	84.45	0.39
Other job lasting >3 months	27.9	31.2	-3.3	106.51	87.67	18.84	71.99	62.12	9.87
Other	26.9	10.5	16.4**	95.34	135.51	-40.17**	67.50	56.22	11.28
School Status at Referral									
Enrolled	27.0	14.0	13.0*	116.55	132.02	-15.47	66.86	59.19	7.67
Not enrolled	31.9	21.4	10.5*	91.36	114.55	-23.19	73.76	63.86	9.90
Total in Sample			395			399			392

NOTE: These results were estimated through ordinary least squares techniques. The control variables included in the models which underlie the overall net impact estimates are defined in Appendix A to the report.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

absence of STETS, as evidenced by the earnings of the control group members. However, participating in STETS raised the average regular-job earnings of males by 144 percent, to $48 per week ($134 per week among those employed). The average regular job earnings of females in the experimental group were comparable to those of their control group counterparts in month 22 (about $21 per week, on average). These differential results for males and females seem to be related to the observed greater difficulty of the programs in placing females in Phase 2 program jobs, especially during the non-steady-state periods of program operations.

2. Training and Schooling

The employment impacts that were estimated for STETS were hypothesized to lead to reductions in the use of other training programs and schooling. Such an outcome is important because many forms of training and schooling for mentally retarded young adults are expensive, long-term programs that do not have a strong record of placing individuals into competitive employment.

A negative program impact on schooling was evident at month 6 as the activities of experimentals were absorbed by STETS; a smaller negative impact persisted in the postprogram period. However, the pattern of the impacts on training is more complicated. Although controls used training programs to a great extent during the early months after their application to STETS, experimentals used such programs to an even greater extent during the in-program period, since STETS, itself, was a training program. However, as expected, STETS did substantially reduce the use of training (primarily workshop-related training) in the postprogram period by an amount (43 percent) that was roughly equal to the postprogram increase in regular employment (see Table IX.1). Thus, the STETS-induced increase in employment did carry with it overall resource-cost savings from the reduced use of training programs and schooling.

3. Public Transfer Use

The hypothesized employment impacts were expected to lead to reductions in the use of public transfers. Although some evidence suggests that STETS did reduce dependence on cash transfers, the results are smaller than were initially expected. The impacts shown in Table IX.1 are reasonably large for the in-program period, with a 26 percent decrease in the receipt of any cash transfers (32 percent for experimentals versus 43 percent for controls). However, this impact faded over time, until no statistically significant impact was evident by month 22 (although the point estimate is a reduction of 3 percentage points in transfer use and an average of nearly $10 per month in benefits).

It is noteworthy that relatively larger persistent reductions were estimated for the proportion of cash transfers accounted for by Supplemental Security Income and Social Security Disability Insurance. (The percentage decrease in the receipt of these transfers hovered around 15 points.) In month 22, the $21 per-month average reduction in SSI/SSDI benefits offset about 40 percent of the average $54 per-month earnings gains; at the same time, benefits from other cash transfers (primarily welfare benefits) increased by 50 percent, from $19 to $29 per month, due at least in part to the increased receipt of unemployment insurance benefits.

The subgroups that were responsible for the small overall reductions in cash transfers are generally those that experienced reductions in SSI/SSDI income. As shown in Table IX.2, these subgroups are often, but not always, the same subgroups that experienced the greatest gains in regular employment from participating in STETS.

4. Economic Status, Independence, and Life-Style

The impacts of the program on employment were also expected to influence socio-economic status, independence, and life-style. STETS did raise the total personal income of participants, but by less than would be indicated by the earnings gains, since the earnings gains coincided with reductions in cash transfer benefits, as was noted above. In the post-program period, STETS increased total personal income by between $8 and $9 (see Table IX.1). Perhaps because of the modest level of this increase in personal income (generally among the same groups who were moving into regular employment, as shown in Table IX.2), we failed to detect any strong patterns of program impacts in the postprogram period on measures of independence and life-style. Admittedly, measures of independence and life-style are less well defined than are measures of the other outcomes under evaluation. However, it is likely that the income gains for most experimentals were too small to effect measurable changes in other aspects of independence or life-style; and for those with more substantial income gains, the observation period may simply have been too short to expect that the gains would translate into long-term adjustments in these areas.

B. RESULTS OF THE BENEFIT-COST ANALYSIS

The benefit-cost analysis suggests that programs such as STETS can be a worthwhile social investment. On average, it cost $9,400 per participant to provide the STETS intervention, of which $3,100 represents wages paid to the participants in Phase 1 and Phase 2 jobs and so not a cost to society.[1] During the 22-month observation period, this investment yielded increases in output and reductions in the use of other programs by participants that offset about 85 percent of this initial investment. Consequently, we observed a measured net social cost of about $1,000 per participant for the period covered by our data. However, trends observed for the impacts on earnings and the use of sheltered workshops suggest that benefits will persist and are likely to outweigh costs in the long-run. If the earnings gains and benefits from the reduced use of alternative programs continued undiminished for as little as seven months beyond the 22-month point, social benefits would exceed social costs. This finding is consistent with other successful training programs that incurred substantial up-front costs for training in order to generate long-run benefits.

In addition, the STETS investment created intangible benefits by increasing the overall employment and social opportunities available to the participants. While the evidence is limited, the fact that many participants tended to remain voluntarily on their jobs suggests that STETS enhanced the quality of their lives. Furthermore, the increased income, regular job-holding, and social interaction provided by STETS are also expected to represent benefits.

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[1] This average cost per participant compares favorably with the average costs of other similar programs targeted toward mentally retarded populations and other disadvantaged groups (see Thornton and Maynard, 1985, for such a comparison).

Because of their increased earnings, both during and after STETS, participants benefitted substantially from STETS. On average, they received a net benefit during the 22-month observation period of $2,100 per participant, and this amount is expected to grow as the participants continue to work.

From the perspective of nonparticipant taxpayers, STETS required a substantial investment of $3,100 per participant during the observation period. This group paid not only the program operating costs, but also the in-program participant-compensation expenditures. Approximately two-thirds of these costs were offset by output produced by participants while in STETS, by savings from the reduced use of sheltered workshops and other programs, and by savings in transfer payments (primarily SSI/SSDI). If these impacts persist at their 22-month levels, then the nonparticipant costs will be entirely recouped within four and a half years after randomization. Even if measured benefits to nonparticipant taxpayers fall short of their costs, STETS may still be an attractive investment, because it effectively achieves a widely stated goal of public policy--to increase the employment opportunities and performance of mentally retarded young adults.

C. OVERALL POLICY CONCLUSIONS

The results of this evaluation demonstrate that many mentally retarded young adults can indeed perform adequately in competitive employment situations, sometimes with minimal or no ongoing support services. More importantly, the results of this study indicate that transitional employment services such as were provided by STETS can be very instrumental in helping mentally retarded young adults achieve their employment potential.

These overall evaluation results compare favorably with employment and training programs targeted on other disadvantaged subgroups of the population. Most notably, the results compare favorably with those of the Supported Work demonstration, which was influential in the design and public support for the STETS demonstration.

These results for the STETS program also strengthen the broad conclusions of the feasibility of implementing transitional and supported employment programs for mentally retarded young adults. More importantly, they provide evidence that implementation on a moderate scale (40 to 50 slots) is feasible at an average cost per participant that is within the range of the cost estimates for both employment and training programs targeted on other segments of the disadvantaged population and alternative programs designed to serve mentally retarded persons.[1]

The specific policy conclusions that evolve from this evaluation of the STETS demonstration pertain to (1) the potential of STETS to mitigate the employment and independence problems of mentally retarded young adults; (2) key features of program design and targeting; and (3) the benefits and costs of the program.

1. Program Potential

STETS-type programs can be expected to have no impact on the overall employment level of mentally retarded young adults who are recruited through methods similar to those used in this demonstration. However, they will tend to move workers out of workshops and into competitive employment. While the net gain in unsubsidized employment attributable to STETS is about 12 percentage points, this overall result represents aggregate impacts and masks the substantially larger effects for selected subgroups of the target population. Even at this apparently modest overall figure, the estimated impacts represent a movement into competitive employment of 60 percent of those who would otherwise be in workshops or activity centers. Furthermore, if one looks at the effects during the steady-state period of operations, when the programs successfully served both males and females, the overall employment impacts are about 25 to 30 percent larger. Moreover, evidence suggests that employment retention will be quite high among those who are successful in making the transition to other employment.

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[1] See further discussions of these cross-program comparisons in Thornton and Maynard (1985). Table A.15 presents estimates of the average costs of STETS and alternative programs.

These programs can be expected to reduce public outlays for SSI/SSDI and education and training services substantially (especially sheltered workshops). However, partially offsetting increases in outlays for other cash transfer programs can also be expected.

No clear evidence exists to indicate that aspects of life-style or social well-being will improve. However, since participation in this program was voluntary, one can assume that most, if not all, participants placed some nonpecuniary value on obtaining regular employment.

2. Program Design and Targeting

Several noteworthy results from this evaluation pertain to program design and targeting issues. First, evidence clearly suggests that ongoing programs might well achieve substantially greater success than was observed for the full STETS demonstration, since we found much larger estimated employment impacts when we compared the employment behavior of experimentals and controls who partcipated in the program during the "steady-state" period--an approximately five-month period when project operations were relatively stable. Second, Phase 2 activities (the supported employment and training experience within a competitive employment setting) were a key part of the program treatment. Third, programs should not "cream" in selecting participants. As has been found in evaluations of employment and training programs targeted toward other segments of the population, STETS was often very successful with those whose prospects for entering competitive employment in the absence of the program were the lowest. One noteworthy characteristic that identified such groups was an IQ score that indicated mild to moderate retardation.

One caveat with respect to the targeting issue is the strikingly more favorable impacts of the program for males than for females, even though both groups are expected to have quite similar success rates in entering competitive employment in the absence of STETS. In view of the estimated importance of Phase 2 activities, the fact that a disproportionate share of female participants relative to males never entered Phase 2 employment may explain, in part, the lack of significant employment impacts for females. Obviously, further investigation of the nature of the greater difficulties in serving females is warranted. However, the results of this study suggest that a major factor that affects the ability of program operators to meet the greater challenge presented by female participants is simply whether their attention is diverted by major program operational changes, such as start-up or phase-down activities; in the steady-state period, their success with females was only slightly lower than their success with males.

3. Benefits and Costs

In the long-run, STETS-type programs are probably a socially justifiable investment, in as much as the total benefits (the net increase in the value of the goods and services produced) will outweigh the costs of the program. However, as is generally the case, taxpayers must be willing to make a substantial up-front investment that may not be repaid to society for several years. Using the full STETS sample and assuming some modest decay (5 percent per year) in the postprogram results, we estimate that this pay-off period to society is about two and a half years, and the pay-off period to nonparticipants (tax payers) would be about four and a half years. However, if the effectiveness of ongoing programs were more similar to the effectiveness of STETS during its "steady-state" operations, this payback period could be substantially shorter.

4. Generalizability of the Findings

In view of the evaluation design, the results of this study are quite robust and provide reliable estimates of what one would expect upon replicating the STETS demonstration. However, the study findings are limited in two respects. First, they are based on only five, judgmentally selected urban sites, where programs were specially designed and implemented for this demonstration. We cannot be certain whether other program operators in other sites and who operate on-going programs under different social, political, and economic conditions would have similar experiences. (For example, economic conditions were generally poor during the period of this demonstration.) It is especially problematic whether similar programs could be efficiently and effectively operated in rural areas or even in more dispersed labor markets. Second, the participants represent only a small portion of the areas' populations of mentally retarded young adults, with most of the screening and selection having occurred at the social service agencies which made referrals to STETS. The evaluation design did not enable us to estimate or, more importantly, to characterize the selection processes and participation decisions. Thus, we are unable to predict the total portion of the target population that could potentially be moved into competitive employment through the provision of STETS-type services.

Despite these limitations with the study, the overall conclusion that transitional employment is an effective program for increasing competitive job-holding among mentally retarded young adults is quite clear. In fact, this program appears to be among the more effective employment and training initiatives that have been field-tested. For example, these results compare favorably with the often-cited examples of successful employment and training programs (Supported Work for the long-term welfare-dependent population and Job Corps),[1] and they are much more favorable than the results for youth-oriented initiatives other than Job Corps.[2]

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[1] See Hollister et al. (1984) and Mallar et al. (1982).

[2] See, for example, Maynard (1984), Bassi and Simms (1983), and Dickinson et al. (1984).

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APPENDIX A

SUPPLEMENTARY TABLES

TABLE A.1

DEFINITIONS AND MEANS OF BASELINE CONTROL VARIABLES USED IN THE ANALYSIS

Baseline Control Variables	Reference Period Month 6	Month 15	Month 22
Site			
Cincinnati	.205	.207	.198
Los Angeles	.201	.214	.213
New York	.244	.239	.237
St. Paul	.127	.124	.129
Tucson	.223	.216	.223
IQ Level[a]			
Borderline	.293	.294	.289
Mild	.583	.599	.600
Moderate	.124	.107	.111
Age			
Younger than 22	.731	.692	.689
22 or older	.269	.308	.311
Gender			
Male	.569	.577	.565
Female	.431	.423	.435
Race/Ethnicity			
Black	.318	.321	.311
Hispanic	.109	.144	.142
White and other	.573	.535	.547
Living Arrangement			
Living with parents	.830	.823	.815
Living in supervised setting[b]	.095	.095	.101
Living independently	.075	.082	.084
Financial Management Skills[c]			
Independent	.291	.274	.274
Not independent	.709	.726	.726
Receipt of Transfers			
SSI/SSDI	.324	.335	.336
Other transfers only[d]	.293	.321	.329
No transfers	.383	.344	.335
Secondary Handicaps[e]			
Secondary handicap	.371	.358	.354
No secondary handicap	.629	.642	.646
Cause of Retardation			
Organic	.164	.163	.171
Non-organic	.836	.837	.829
Benefactor[f]			
Benefactor	.286	.291	.296
No benefactor	.714	.709	.829
Work Experience in Two Years Prior to Enrollment			
Regular job lasting >3 months	.148	.147	.147
Other job lasting >3 months	.360	.348	.342
Other	.492	.505	.511

TABLE A.1 (continued)

Baseline	Reference Period		
Control Variables	Month 6	Month 15	Month 22
School Status at Referral			
Enrolled	.304	.312	.310
Not enrolled	.696	.688	.690
Experimental Status			
Experimental	.512	.507	.522
Control	.488	.493	.478
Number in Sample	283	402	395

NOTE: All control variables are measured at baseline or referral. These means pertain to those sample members included in the Employment Outcome models. The following data items were obtained from the Application/Enrollment form, which was completed by the referral agencies: IQ level, age, gender, race/ethnicity, secondary handicap, cause of retardation, prior work experience, and school status at referral. The other data items were collected during the baseline interview.

[a] The three classifications of IQ level are defined as follows: borderline is from one to two standard deviations (SD) below the mean on tests, mild is from two to three SD below the mean, and moderate is from three to four SD below the mean. IQ scores on the Wechsler scales (SD=15) which fall within these ranges are: borderline 70-84, mild 55-69, moderate 40-54. The ranges for scores on the Stanford-Binet test (SD=16) are slightly different. For some sample members, only the range of retardation, and not IQ score, was reported. The American Association of Mental Deficiency no longer recognizes the borderline classification, but the term is used in this report to classify individuals with test scores above the mild range.

[b] Supervised settings include institutions, group homes, supervised apartments, and other semi-independent settings.

[c] Independence is defined as performing without assistance at least two financial management activities (shopping, handling bills, and using bank accounts), and without assistance in the other financial management activity.

[d] Other transfers include Medicare, Medicaid, AFDC, General Assistance, or Social Security received by or on behalf of the sample member, and food stamps received by the sample member's household.

[e] Secondary handicaps include: seizure disorders, visual impairment, emotional impairment, cerebral palsy, specific learning disability, mobility limitation, and others. Note that STETS clients were to have no secondary disability that would make on-the-job training for competitive employment impractical (see the discussion of the program eligibility criteria in Chapter II).

[f] Benefactors are defined as individuals named by the primary sample member as providing assistance in two or more of the following areas: job search, residential counseling, financial management (including helping with the receipt of transfer payments), other counseling, and transportation. These individuals could be relatives or friends of the sample member, or service agency staff members.

TABLE A.2

ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE IV.4, IV.6, AND A.4

Control Variables	Outcome Measure and Time Period			
	Earnings in Regular Jobs		Percent With Any Paid Job	
	Month 6	Month 22	Month 6	Month 22
Intercept	7.1	37.7	68.7	0.7
Site				
Cincinnati	-12.4	-44.1	-21.0	-40.6
Los Angeles	-16.6	-43.4	-21.7	-23.2
New York	—	—	—	—
St. Paul	-19.1	-31.6	-3.7	14.4
Tucson	0.6	-14.5	-2.1	3.8
IQ Level				
Borderline	7.3	10.2	11.9	3.7
Mild	—	—	—	—
Moderate	9.7	-4.5	16.4	17.4
Age				
Younger than 22	6.2	15.9	-14.3	-6.7
22 or older	—	—	—	—
Gender				
Male	—	—	—	—
Female	-0.4	2.1	6.8	-5.9
Race/Ethnicity				
Black	-1.3	-5.2	-11.2	-12.3
Hispanic	-1.2	8.7	-15.5	-11.1
White and other	—	—	—	—
Living Arrangement				
Living with parents	6.0	-5.9	2.7	-6.6
Living in supervised setting	—	—	—	—
Living independently	-16.0	-4.1	-45.2	-35.4
Financial Management Skills				
Independent	7.4	7.5	2.3	-2.9
Not independent	—	—	—	—
Receipt of Transfers				
SSI/SSDI	-4.3	-7.9	6.2	1.7
Other transfers only	0.8	-8.3	-2.8	-2.9
No transfers	—	—	—	—
Secondary Handicaps				
Secondary handicap	-7.7	-4.7	-8.6	-5.6
No secondary handicap	—	—	—	—
Cause of Retardation				
Organic	13.6	-5.2	1.9	-3.3
Non-organic	—	—	—	—
Benefactor				
Benefactor	12.3	1.5	-6.6	3.0
No benefactor	—	—	—	—
Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	1.6	22.8	-3.0	9.7
Other job lasting ≥3 months	0.4	16.1	8.0	21.3
Other	—	—	—	—
School Status at Referral				
Enrolled	-14.4	-13.7	-17.0	-6.5
Not enrolled	—	—	—	—

TABLE A.2 (continued)

	Outcome Measure and Time Period			
	Earnings in Regular Jobs		Percent With Any Paid Job	
Control Variables	Month 6	Month 22	Month 6	Month 22
Experimental Status				
Experimental	-3.8	-5.9	27.7	-23.0
Control	--	--	--	--
E*Site				
Cincinnati	18.1	-0.4	-6.4	17.0
Los Angeles	30.7	9.6	-16.2	2.7
New York	--	--	--	--
St. Paul	42.7	12.2	-4.9	-2.2
Tucson	3.7	-14.6	19.1	-2.8
E*IQ Level				
Borderline	-5.0	4.5	-10.9	-2.2
Mild	--	--	--	--
Moderate	-14.0	17.2	-22.2	-3.0
E*Age				
Younger than 22	-8.6	-17.4	11.4	-3.2
22 or older	--	--	--	--
E*Gender				
Male	--	--	--	--
Female	-18.1	-28.6	-12.1	-11.4
E*Race/Ethnicity				
Black	11.7	2.4	10.6	1.1
Hispanic	6.2	12.9	34.0	29.4
White and other	--	--	--	--
E*Living Arrangement				
Living with parents	-1.1	24.3	-17.0	19.3
Living in supervised setting	--	--	--	--
Living independently	41.4	23.3	35.0	23.2
E*Financial Management Skills				
Independent	-9.1	19.5	-4.8	12.0
Not independent	--	--	--	--
E*Receipt of Transfers				
SSI/SSDI	-6.0	20.5	4.7	5.1
Other transfers only	-5.8	30.3	14.0	20.6
No transfers	--	--	--	--
E*Secondary Handicaps				
Secondary handicap	--	--	--	--
No secondary handicap	--	--	--	--
E*Cause of Retardation				
Organic	-11.0	10.1	-8.1	15.0
Non-organic	--	--	--	--
E*Benefactor				
Benefactor	--	--	--	--
No benefactor	--	--	--	--
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	12.7	1.1	14.4	-1.0
Other job lasting >3 months	5.6	-20.9	-10.5	-18.6
Other	--	--	--	--
E*School Status at Referral				
Enrolled	14.8	4.6	5.8	-2.9
Not Enrolled	--	--	--	--

NOTE: These results were estimated through ordinary least squares techniques. All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. **E*** indicates that a control variable has been interacted with **Experimental Status**. The t-statistics for the coefficient estimates are reported in Table A.3.

TABLE A.3

t-STATISTICS FOR ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE IV.4, IV.6, AND A.4

	Outcome Measure and Time Period			
	Earnings in Regular Jobs		Whether Any Paid Job	
Control Variables	Month 6	Month 22	Month 6	Month 22
Intercept	0.38	1.67	2.58	3.45
Site				
Cincinnati	-1.38	-3.46	-1.64	-3.65
Los Angeles	-1.87	-3.64	-1.72	-2.23
New York	--	--	--	--
St. Paul	-1.61	-2.05	-0.22	1.07
Tucson	0.06	-1.13	-0.15	0.34
IQ Level				
Borderline	1.12	1.14	1.28	0.47
Mild	--	--	--	--
Moderate	0.93	-0.31	1.10	1.38
Age				
Younger than 22	0.74	1.48	-1.20	-0.72
22 or older	--	--	--	--
Gender				
Male	--	--	--	--
Female	-0.07	0.26	0.79	-0.84
Race/Ethnicity				
Black	-0.18	-0.50	-1.06	-1.36
Hispanic	-0.12	0.69	-1.05	-1.01
White and other	--	--	--	--
Living Arrangement				
Living with parents	0.51	-0.38	0.16	-0.48
Living in supervised setting	--	--	--	--
Living independently	-0.84	-0.19	-1.67	-1.87
Financial Management Skills				
Independent	1.09	0.78	0.24	-0.34
Not independent	--	--	--	--
Receipt of Transfers				
SSI/SSDI	-0.53	-0.70	0.54	0.17
Other transfers only	0.11	0.82	-0.26	-0.33
No transfers	--	--	--	--
Secondary Handicaps				
Secondary handicap	-1.73	-0.79	-1.35	-1.09
No secondary handicap	--	--	--	--
Cause of Retardation				
Organic	1.54	-0.43	0.16	-0.31
Non-organic	--	--	--	--
Benefactor				
Benefactor	2.41	0.23	-0.90	0.50
No benefactor	--	--	--	--
Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	0.18	1.84	-0.24	0.90
Other job lasting ≥3 months	0.06	1.84	0.87	2.79
Other	--	--	--	--
School Status at Referral				
Enrolled	-2.07	-1.41	-1.71	-0.76
Not enrolled	--	--	--	--

TABLE A.3 (continued)

Control Variables	Outcome Measure and Time Period Earnings in Regular Jobs Month 6	 Month 22	 Whether Any Paid Job Month 6	 Month 22
Experimental Status				
Experimental	-0.16	-0.20	0.84	-0.90
Control	--	--	--	--
E*Site				
Cincinnati	1.40	-0.02	-0.35	1.11
Los Angeles	2.41	0.57	-0.89	0.18
New York	--	--	--	--
St. Paul	2.59	0.56	-0.21	-0.12
Tucson	0.27	-0.81	0.98	-0.18
E*IQ Level				
Borderline	-0.53	0.35	-0.82	-0.19
Mild	--	--	--	--
Moderate	-0.99	0.91	-1.11	-0.18
E*Age				
Younger than 22	-0.79	-1.25	0.74	-0.26
22 or older	--	--	--	--
E*Gender				
Male	--	--	--	--
Female	-2.11	-2.54	-0.99	-1.16
E*Race/Ethnicity				
Black	1.12	0.17	0.72	0.09
Hispanic	0.42	0.73	1.61	1.91
White and other	--	--	--	--
E*Living Arrangement				
Living with parents	-0.72	1.20	-0.77	1.10
Living in supervised setting	--	--	--	--
Living independently	1.81	0.85	1.07	0.97
E*Financial Management Skills				
Independent	-0.92	1.48	-0.34	1.04
Not independent	--	--	--	--
E*Receipt of Transfers				
SSI/SSDI	-0.54	1.36	0.30	0.39
Other transfers only	-0.55	2.18	0.92	1.70
No transfers	--	--	--	--
E*Secondary Handicaps				
Secondary handicap	--	--	--	--
No secondary handicap	--	--	--	--
E*Cause of Retardation				
Organic	-0.91	0.65	-0.47	1.11
Non-organic	--	--	--	--
E*Benefactor				
Benefactor	--	--	--	--
No benefactor	--	--	--	--
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	1.00	0.07	0.80	-0.07
Other job lasting ≥3 months	0.62	-1.75	-0.83	-1.79
Other	--	--	--	--
E*School Status at Referral				
Enrolled	1.46	0.33	0.40	-0.24
Not enrolled	--	--	--	--

NOTE: All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. E* indicates that a control variable has been interacted with **Experimental Status.** The corresponding coefficient estimates are reported

TABLE A.4

ESTIMATED PROGRAM IMPACTS ON EARNINGS FOR KEY SUBGROUPS OF STETS PARTICIPANTS

A. EARNINGS FROM REGULAR JOBS[a]

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	11.81	9.81	2.00	26.90	16.31	10.59**	36.36	20.55	15.81**
Site									
Cincinnati	9.56	5.57	3.99	19.33	6.67	12.66	16.79	1.70	15.09
Los Angeles	17.93	1.36	16.57*	14.27	-1.47	15.74	27.55	2.42	25.13**
New York	3.85	17.99	-14.14	50.44	42.48	7.96	61.31	45.79	15.52
St. Paul	27.58	-1.06	28.64**	27.00	13.30	13.70	41.90	14.20	27.70*
Tucson	8.14	18.61	-10.47	20.59	15.94	4.65	32.25	31.28	0.97
IQ Level									
Borderline	13.98	13.76	0.22	26.90	29.69	-2.79	45.44	28.32	17.12
Mild	11.66	6.47	5.19	24.45	10.25	14.20**	30.68	18.09	12.59*
Moderate	7.42	16.18	-8.76	40.71	13.58	27.13*	43.42	13.63	29.79*
Age									
Younger than 22	11.17	11.47	-0.30	27.83	20.47	7.36	35.89	25.51	10.38
22 or older	13.56	5.29	8.27	24.83	6.99	17.84*	37.41	9.58	27.83**
Gender									
Male	19.80	9.99	9.81*	32.32	17.13	15.19**	47.89	19.61	28.28**
Female	6.27	9.57	-3.30	19.52	15.19	4.33	21.39	21.75	-0.36
Race/Ethnicity									
Black	18.33	9.05	9.28	16.04	13.82	2.22	31.33	15.72	15.61
Hispanic	12.96	9.15	3.81	34.51	30.91	3.60	55.85	29.67	26.18*
White and other	7.68	10.36	-2.68	31.38	13.87	17.51**	34.17	20.93	13.24
Living Arrangement									
Living with parents	10.77	12.04	-1.27	27.11	15.48	11.63**	38.17	19.81	18.36**
Living in supervised setting	5.83	5.99	-0.16	10.50	16.54	-6.04	19.72	25.67	-5.95
Living independently	31.24	-10.03	41.27**	43.83	24.41	19.42	38.88	21.52	17.36
Financial Management Skills									
Independent	10.62	15.04	-4.42	41.53	14.86	26.67**	55.98	25.99	29.99**
Not independent	12.31	7.67	4.64	21.38	16.86	4.52	28.95	18.49	10.46
Receipt of Transfers									
SSI/SSDI	6.36	6.68	-0.32	22.07	16.11	5.96	37.48	18.03	19.45*
Other transfers only	11.62	11.79	-0.17	35.96	14.23	21.73**	46.92	17.63	29.29**
No transfers	16.58	10.95	5.63	23.17	18.44	4.73	24.87	25.93	-1.06
Cause of Retardation									
Organic	14.00	21.16	-7.16	17.17	13.40	3.77	40.42	16.24	24.18*
Non-organic	11.39	7.59	3.80	28.80	16.88	11.92**	35.52	21.43	14.09**
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	21.81	11.00	10.81	43.47	27.83	15.64	58.37	34.47	23.90
Other job lasting >3 months	12.52	9.85	2.67	24.73	15.76	8.97	29.70	27.82	1.88
Other	7.55	9.43	-1.88	23.59	13.35	10.24*	34.49	11.69	22.80**
School Status at Referral									
Enrolled	12.64	-0.20	12.84	22.04	5.42	16.62*	30.06	11.08	18.98*
Not enrolled	11.67	14.18	-2.51	29.12	21.25	7.87	39.18	24.79	14.39**
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table IV.4, these models include variables that interacted the treatment variable with the subgroup variables. The full regression from which the 6- and 22-month results were derived are presented in Tables A.2 and A.3.

[a] Regular jobs are those that are neither training/work-study nor workshop/activity-center jobs.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

TABLE A.4

ESTIMATED PROGRAM IMPACTS ON EARNINGS FOR KEY SUBGROUPS OF STETS PARTICIPANTS

B. EARNINGS FROM ALL PAID JOBS

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	52.39	25.93	26.46**	37.91	26.48	11.43**	40.79	28.41	12.38**
Site									
Cincinnati	40.27	15.49	24.78*	21.38	7.96	13.42	21.54	3.74	17.80
Los Angeles	39.64	13.49	26.15**	24.00	7.53	16.47	28.74	8.24	20.50*
New York	72.32	44.78	27.54**	57.72	56.36	1.36	63.60	52.29	11.31
St. Paul	51.23	22.75	28.48*	36.01	31.49	4.52	48.60	38.48	10.12
Tucson	53.88	27.94	25.94**	46.63	27.02	19.61*	40.46	38.19	2.27
IQ Level									
Borderline	58.59	25.51	33.08**	38.55	35.36	3.19	47.37	33.06	14.31
Mild	51.80	24.94	26.86**	34.86	22.06	12.80**	36.33	27.50	8.83
Moderate	40.39	31.55	8.84	53.20	26.86	26.34*	47.74	21.25	26.49
Age									
Younger than 22	53.66	25.22	28.44**	39.87	24.60	15.27**	40.41	31.29	9.12
22 or older	48.94	27.87	21.07*	33.49	30.68	2.81	41.61	22.02	19.59*
Gender									
Male	57.20	21.59	35.61**	48.16	29.10	19.06**	53.55	28.00	25.55**
Female	46.05	31.67	14.38*	23.90	22.90	1.00	24.23	28.93	-4.70
Race/Ethnicity									
Black	55.27	23.49	31.78**	32.09	22.79	9.30	34.93	23.87	11.06
Hispanic	68.36	30.52	37.84**	53.83	47.52	6.31	62.86	37.47	25.39*
White and other	47.73	26.41	21.32**	37.09	23.01	14.08*	38.40	28.64	9.76
Living Arrangement									
Living with parents	50.58	27.89	22.69**	37.05	27.28	9.77*	42.29	27.38	14.91**
Living in supervised setting	57.29	26.28	31.01	31.83	25.67	6.16	31.55	35.56	-4.01
Living independently	66.14	3.59	62.55**	53.41	19.36	34.05*	37.35	29.70	7.65
Financial Management Skills									
Independent	55.56	31.96	23.60**	56.79	29.95	26.84**	60.51	29.67	30.84**
Not independent	51.09	23.46	27.63**	30.77	25.17	5.60	33.34	27.93	5.41
Receipt of Transfers									
SSI/SSDI	56.90	25.54	31.36**	35.35	24.03	11.32	42.93	25.82	17.11
Other transfers only	50.55	23.80	26.75**	44.75	23.69	21.06**	50.43	24.25	26.18**
No transfers	49.98	27.89	22.09**	34.01	31.45	2.56	29.19	35.08	-5.89
Cause of Retardation									
Organic	47.65	30.77	16.88	36.79	13.20	23.59*	46.07	22.46	23.61*
Non-organic	53.31	24.98	28.33**	38.12	29.05	9.07*	39.70	29.63	10.07*
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	64.09	28.63	35.46**	55.49	40.10	15.39	60.55	41.89	18.66
Other job lasting >3 months	48.35	27.89	20.46**	35.99	31.79	4.20	34.72	37.71	-2.99
Other	51.82	23.68	28.14**	34.11	18.85	15.26**	(2.53)	18.32	-20.85**
School Status at Referral									
Enrolled	48.86	14.65	34.21**	26.98	22.04	4.94	31.89	19.66	12.23
Not enrolled	53.93	30.86	23.07**	42.87	28.50	14.37**	44.78	32.33	12.45*
Number in Sample			283			402			395

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates reported in Table IV.4, these models include variables that interacted the treatment variable with the subgroup variables.

*Statistically significant at the 10 percent level, two-tailed test.

**Statistically significant at the 5 percent level, two-tailed test.

TABLE A.5

COMPARISON OF SELECTED NET IMPACT ESTIMATES BASED ON OLS REGRESSION MODELS AND MAXIMUM LIKELIHOOD ESTIMATION METHODS

Reference Period/ Estimation Method	Outcome Measure			
	Regular Jobs	Training Jobs	Workshop/ Activity Center	Any Paid Job
Month 6				
OLS	1.1	27.2**	-6.3*	22.6**
Probit	-0.2	30.4**	-4.8*	24.1**
Month 22				
OLS	11.9**	0.3	-11.3**	1.0
Probit	13.5**	0.1	-9.2**	0.1

NOTE: The OLS estimates are based on the basic regression model specified in Appendix Table A.1. The impact estimate based on this model is the coefficient on the experimental status variable. The probit estimates are based on maximum likelihood estimates of coefficients on a similar set of control variables as was used in the OLS model. In this case, the experimental effect is estimated as:

$$\text{PROB } (\overline{X}B \mid \text{experimental}) - \text{PROB } (\overline{X}B \mid \text{control}),$$

where PROB ( ) is the probability.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.6

CHOW TEST RESULTS OF THE ACCEPTABILITY OF POOLING OBSERVATIONS ACROSS SITES

Outcome Measure	F-Statistic	Degrees of Freedom
Total Earnings in Month 6	1.27	86, 192
Regular Job Earnings in Month 6	1.19	86, 192
Total Earnings in Month 22	1.06	86, 304
Regular Job Earnings in Month 22	1.10	86, 304

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.7

ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE V.4 AND V.5

	Outcome Measure and Time Period			
	Percent in Training		Percent in School	
Control Variables	Month 6	Month 22	Month 6	Month 22
Intercept	82.6	54.5	-7.2	-11.2
Site				
Cincinnati	-22.2	-2.6	-3.1	-4.9
Los Angeles	-7.1	0.1	14.6	13.3
New York	—	—	—	—
St. Paul	-2.9	6.5	4.8	-6.7
Tucson	-5.0	1.6	-0.1	3.9
IQ Level				
Borderline	-1.3	-11.3	-7.3	-0.6
Mild	—	—	—	—
Moderate	6.4	-9.1	-6.8	12.0
Age				
Younger than 22	-21.4	-21.1	-1.5	13.2
22 or older	—	—	—	—
Gender				
Male	—	—	—	—
Female	1.0	0.4	1.0	1.2
Race/Ethnicity				
Black	-8.1	-12.4	0.6	-12.0
Hispanic	-12.7	-19.0	0.8	-17.2
White and other	—	—	—	—
Living Arrangement				
Living with parents	-25.5	-0.6	22.1	15.4
Living in supervised setting	—	—	—	—
Living independently	-51.6	-23.0	-0.3	5.1
Financial Management Skills				
Independent	5.7	-9.2	15.2	-0.8
Not independent	—	—	—	—
Receipt of Transfers				
SSI/SSDI	20.1	6.8	-3.2	8.8
Other transfers only	10.9	6.3	-5.5	0.4
No transfers	—	—	—	—
Secondary Handicaps				
Secondary handicap	2.1	-6.8	-7.5	3.1
No secondary handicap	—	—	—	—
Cause of Retardation				
Organic	-7.3	23.3	8.7	-5.2
Non-organic	—	—	—	—
Benefactor				
Benefactor	-16.8	0.7	1.9	-2.2
No benefactor	—	—	—	—
Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	5.2	-3.5	-3.6	-10.4
Other job lasting ≥3 months	13.9	-4.7	2.7	-3.4
Other	—	—	—	—
School Status at Referral				
Enrolled	-9.2	0.1	12.1	10.9
Not enrolled	—	—	—	—
Experimental Status				
Experimental	2.0	-19.4	15.0	6.9
Control	—	—	—	—

TABLE A.7 (continued)

	Outcome Measure and Time Period			
	Percent in Training		Percent in School	
Control Variables	Month 6	Month 22	Month 6	Month 22
E*Site				
Cincinnati	-3.6	1.1	-1.8	7.5
Los Angeles	-35.2	2.7	-13.8	-1.1
New York	--	--	--	--
St. Paul	-33.0	4.9	-5.5	6.3
Tucson	12.7	7.7	-0.3	1.9
E*IQ Level				
Borderline	-9.8	9.4	15.5	-4.7
Mild	--	--	--	--
Moderate	-1.5	14.4	11.4	-21.0
E*Age				
Younger than 22	23.1	16.6	1.6	-10.3
22 or older	--	--	--	--
E*Gender				
Male	--	--	--	--
Female	10.1	-4.4	-2.9	4.0
E*Race/Ethnicity				
Black	4.4	4.5	2.3	14.9
Hispanic	20.4	10.8	14.9	13.6
White and other	--	--	--	--
E*Living Arrangement				
Living with parents	7.7	-8.5	-17.9	-11.8
Living in supervised setting	--	--	--	--
Living independently	17.8	-0.5	14.3	-11.5
E*Financial Management Skills				
Independent	2.5	2.3	-18.5	2.7
Not independent	--	--	--	--
E*Receipt of Transfers				
SSI/SSDI	-6.4	0.4	-5.7	-2.8
Other transfers only	7.5	-10.8	7.1	1.7
No transfers	--	--	--	--
E*Secondary Handicaps				
Secondary handicap	--	--	--	--
No secondary handicap	--	--	--	--
E*Cause of Retardation				
Organic	10.6	-15.8	-7.5	7.0
Non-organic	--	--	--	--
E*Benefactor				
Benefactor	--	--	--	--
No benefactor	--	--	--	--
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	-3.7	-8.5	-0.1	4.1
Other job lasting >3 months	-9.4	-6.0	-9.8	5.0
Other	--	--	--	--
E*School Status at Referral				
Enrolled	11.8	-4.4	-12.5	-11.8
Not enrolled	--	--	--	--

NOTE: All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. **E*** indicates that a control variable has been interacted with **Experimental Status.** The t-statistics for the coefficient estimates are reported in Table A.8.

TABLE A.8

t-STATISTICS FOR ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE V.4 AND V.5

Control Variables	Outcome Measure and Time Period			
	Percent in Training		Percent in School	
	Month 6	Month 22	Month 6	Month 22
Intercept	3.12	3.17	-0.39	-0.91
Site				
Cincinnati	-1.75	-0.27	-0.35	-0.71
Los Angeles	-0.56	0.02	1.65	2.04
New York	--	--	--	--
St. Paul	-0.17	0.55	0.41	-0.79
Tucson	-0.36	0.16	-0.02	0.55
IQ Level				
Borderline	-0.14	-1.66	-1.12	-0.12
Mild	--	--	--	--
Moderate	0.44	-0.83	-0.65	1.53
Age				
Younger than 22	-1.81	-2.58	-0.19	2.24
22 or older	--	--	--	--
Gender				
Male	--	--	--	--
Female	0.11	-0.06	0.16	0.27
Race/Ethnicity				
Black	-0.76	-1.57	0.09	-2.12
Hispanic	-0.86	-1.97	0.08	-2.47
White and other	--	--	--	--
Living Arrangement				
Living with parents	-1.51	-0.05	1.85	1.81
Living in supervised setting	--	--	--	--
Living independently	-1.92	-1.39	-0.02	0.43
Financial Management Skills				
Independent	0.60	-1.26	2.26	-0.16
Not independent	--	--	--	--
Receipt of Transfers				
SSI/SSDI	1.78	0.80	-0.40	1.44
Other transfers only	1.01	0.82	-0.73	0.07
No transfers	--	--	--	--
Secondary Handicaps				
Secondary handicap	0.34	-1.51	-1.69	0.96
No secondary handicap	--	--	--	--
Cause of Retardation				
Organic	-0.59	2.56	0.99	-0.80
Non-organic	--	--	--	--
Benefactor				
Benefactor	-2.32	0.13	0.68	-0.59
No benefactor	--	--	--	--
Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	0.41	-0.37	-0.41	-1.54
Other job lasting >3 months	1.53	-0.70	0.42	-0.70
Other	--	--	--	--
School Status at Referral				
Enrolled	-0.93	0.02	1.74	2.05
Not enrolled	--	--	--	--
Experimental Status				
Experimental	0.06	-0.88	0.65	0.43
Control	--	--	--	--

TABLE A.8 (continued)

	Outcome Measure and Time Period			
	Percent in Training		Percent in School	
Control Variables	Month 6	Month 22	Month 6	Month 22
E*Site				
Cincinnati	-0.19	0.08	-0.14	0.78
Los Angeles	-1.95	0.21	-0.19	-0.12
New York	—	—	—	—
St. Paul	-1.42	0.30	-0.34	0.53
Tucson	0.66	0.57	-0.02	0.20
E*IQ Level				
Borderline	-0.74	0.97	1.65	-0.67
Mild	—	—	—	—
Moderate	-0.08	1.00	0.81	-2.03
E*Age				
Younger than 22	1.50	1.57	0.15	-1.34
22 or older	—	—	—	—
E*Gender				
Male	—	—	—	—
Female	0.83	-0.52	-0.34	0.65
E*Race/Ethnicity				
Black	0.30	0.42	0.22	1.91
Hispanic	0.97	0.81	1.00	1.41
White and other	—	—	—	—
E*Living Arrangement				
Living with parents	0.35	-0.56	-1.17	-1.07
Living in supervised setting	—	—	—	—
Living independently	0.55	-0.03	0.63	-0.77
E*Financial Management Skills				
Independent	0.18	0.23	-1.88	0.38
Not independent	—	—	—	—
E*Receipt of Transfers				
SSI/SSDI	-0.41	0.04	-0.51	-0.34
Other transfers only	0.49	-1.03	0.67	0.23
No transfers	—	—	—	—
E*Secondary Handicaps				
Secondary handicap	—	—	—	—
No secondary handicap	—	—	—	—
E*Cause of Retardation				
Organic	-0.62	-1.34	-0.62	0.83
Non-organic	—	—	—	—
E*Benefactor				
Benefactor	—	—	—	—
No benefactor	—	—	—	—
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	-0.21	-0.67	-0.01	0.44
Other job lasting >3 months	-0.74	0.66	-1.09	0.77
Other	—	—	—	—
E*School Status at Referral				
Enrolled	0.82	-0.42	-1.23	-1.56
Not Enrolled	—	—	—	—

NOTE: All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. **E*** indicates that a control variable has been interacted with **Experimental Status.** The t-statistics for the coefficient estimates are reported in Table A.7.

TABLE A.9

ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE VI.3 AND VI.4

	Outcome Measure and Time Period			
	Monthly Income from SSI/SSDI		Monthly Income from Other Cash Transfers	
Control Variables	Month 6	Month 22	Month 6	Month 22
Intercept	-30.3	111.4	52.6	12.3
Site				
Cincinnati	15.4	-21.0	-15.2	16.4
Los Angeles	69.8	81.3	-36.6	-3.7
New York	—	—	—	—
St. Paul	-8.3	-95.5	31.3	29.4
Tucson	42.5	-29.2	-33.8	11.0
IQ Level				
Borderline	0.5	-60.4	-11.7	3.0
Mild	—	—	—	—
Moderate	20.0	-58.6	-7.3	-14.0
Age				
Younger than 22	25.2	-25.8	-22.3	-23.4
22 or older	—	—	—	—
Gender				
Male	—	—	—	—
Female	7.1	-31.7	-12.5	12.6
Race/Ethnicity				
Black	-24.7	-49.2	-11.1	24.5
Hispanic	-22.6	-47.1	-9.9	10.4
White and other	—	—	—	—
Living Arrangement				
Living with parents	-3.4	8.3	-25.9	-22.4
Living in supervised setting	—	—	—	—
Living independently	-5.9	126.4	-50.5	-41.2
Financial Management Skills				
Independent	7.9	3.3	27.0	31.8
Not independent	—	—	—	—
Receipt of Transfers				
SSI/SSDI	180.1	116.9	14.7	6.1
Other transfers only	3.7	50.5	63.5	29.6
No transfers	—	—	—	—
Secondary Handicaps				
Secondary handicap	10.5	32.7	8.5	11.0
No secondary handicap	—	—	—	—
Cause of Retardation				
Organic	-50.8	80.1	2.8	-11.2
Non-organic	—	—	—	—
Benefactor				
Benefactor	18.0	7.1	6.0	-15.0
No benefactor	—	—	—	—
Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	8.4	5.7	-3.2	22.7
Other job lasting >3 months	16.8	-47.8	-1.0	-9.4
Other	—	—	—	—
School Status at Referral				
Enrolled	-3.4	17.5	15.1	14.4
Not enrolled	—	—	—	—
Experimental Status				
Experimental	6.8	-92.4	-65.5	-38.3
Control	—	—	—	—

TABLE A.9 (continued)

	Outcome Measure and Time Period			
	Monthly Income from SSI/SSDI		Monthly Income from Other Cash Transfers	
Control Variables	Month 6	Month 22	Month 6	Month 22
E*Site				
Cincinnati	-16.8	3.0	-1.6	-2.2
Los Angeles	-22.7	13.9	17.2	-2.8
New York	--	--	--	--
St. Paul	-38.7	59.6	21.9	7.3
Tucson	-25.9	37.5	7.8	-23.8
E*IQ Level				
Borderline	-6.4	52.0	7.8	-2.0
Mild	--	--	--	--
Moderate	11.8	92.4	19.4	26.1
E*Age				
Younger than 22	-44.2	-17.5	34.4	39.8
22 or older	--	--	--	--
E*Gender				
Male	--	--	--	--
Female	-21.8	49.4	24.4	9.4
E*Race/Ethnicity				
Black	-19.0	-1.6	0.9	6.6
Hispanic	-14.4	23.4	7.9	-18.1
White and other	--	--	--	--
E*Living Arrangement				
Living with parents	41.9	15.7	28.7	29.3
Living in supervised setting	--	--	--	--
Living independently	115.9	-107.7	48.1	57.6
E*Financial Management Skills				
Independent	-8.4	-12.3	-25.4	-16.6
Not independent	--	--	--	--
E*Receipt of Transfers				
SSI/SSDI	-13.0	50.0	1.5	-3.8
Other transfers only	9.4	-32.7	-32.9	-25.9
No transfers	--	--	--	--
E*Secondary Handicaps				
Secondary handicap	--	--	--	--
No secondary handicap	--	--	--	--
E*Cause of Retardation				
Organic	79.5	-87.7	0.1	5.2
Non-organic	--	--	--	--
E*Benefactor				
Benefactor	--	--	--	--
No benefactor	--	--	--	--
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	-10.5	-4.9	-12.7	-32.2
Other job lasting >3 months	-1.5	59.0	10.0	34.1
Other	--	--	--	--
E*School Status at Referral				
Enrolled	11.1	7.7	-19.2	-6.8
Not enrolled	--	--	--	--

NOTE: All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. **E*** indicates that a control variable has been interacted with **Experimental Status**. The t-statistics for the coefficient estimates are reported in Table A.10.

TABLE A.10

t-STATISTICS FOR ESTIMATED COEFFICIENTS IN MODELS USED TO GENERATE SELECTED NET IMPACT ESTIMATES REPORTED IN TABLE VI.3 AND VI.4

	Outcome Measure and Time Period			
	Monthly Income from SSI/SSDI		Monthly Income from Other Cash Transfers	
Control Variables	Month 6	Month 22	Month 6	Month 22
Intercept	-0.54	1.99	1.46	0.40
Site				
Cincinnati	0.58	-0.67	-0.88	0.95
Los Angeles	2.66	2.72	-2.16	-0.23
New York	--	--	--	--
St. Paul	-0.24	-2.54	1.39	1.40
Tucson	1.47	-0.92	-1.82	0.63
IQ Level				
Borderline	0.02	-2.74	-0.94	0.25
Mild	--	--	--	--
Moderate	0.64	-1.67	-0.37	-0.64
Age				
Younger than 22	1.02	-0.97	-1.40	-1.62
22 or older	--	--	--	--
Gender				
Male	--	--	--	--
Female	0.40	-1.59	-1.08	1.15
Race/Ethnicity				
Black	-1.13	-1.93	-0.79	1.75
Hispanic	-0.75	-1.51	-0.51	0.60
White and other	--	--	--	--
Living Arrangement				
Living with parents	-0.09	0.22	-1.09	-1.08
Living in supervised setting	--	--	--	--
Living independently	-0.10	2.31	-1.38	-1.40
Financial Management Skills				
Independent	0.39	0.14	2.04	2.42
Not independent	--	--	--	--
Receipt of Transfers				
SSI/SSDI	7.66	4.30	0.97	0.41
Other transfers only	0.17	2.05	4.39	2.17
No transfers	--	--	--	--
Secondary Handicaps				
Secondary handicap	0.79	2.25	1.00	1.38
No secondary handicap	--	--	--	--
Cause of Retardation				
Organic	-1.97	2.75	0.17	-0.20
Non-organic	--	--	--	--
Benefactor				
Benefactor	1.19	0.42	0.62	-1.62
No benefactor	--	--	--	--
Work Experience in Two Years Prior to Enrollment				
Regular job lasting ≥3 months	0.31	0.19	-0.18	1.36
Other job lasting ≥3 months	0.88	-2.22	-0.08	-0.79
Other	--	--	--	--
School Status at Referral				
Enrolled	-0.17	0.73	1.14	1.09
Not enrolled	--	--	--	--
Experimental Status				
Experimental	0.10	-1.28	-1.46	-0.97
Control	--	--	--	--

TABLE A.10 (continued)

Control Variables	Outcome Measure and Time Period Monthly Income from SSI/SSDI Month 6	 Month 22	 Monthly Income from Other Cash Transfers Month 6	 Month 22
E*Site				
Cincinnati	-0.44	0.07	-0.07	-0.09
Los Angeles	-0.60	0.33	0.71	-0.12
New York	--	--	--	--
St. Paul	-0.79	1.13	0.69	0.25
Tucson	-0.64	0.85	0.30	0.98
E*IQ Level				
Borderline	-0.23	1.66	0.44	-0.12
Mild	--	--	--	--
Moderate	0.28	1.97	0.72	1.03
E*Age				
Younger than 22	-1.36	-0.51	1.66	2.11
22 or older	--	--	--	--
E*Gender				
Male	--	--	--	--
Female	-0.86	1.79	1.50	0.62
E*Race/Ethnicity				
Black	-0.61	-0.05	0.04	0.35
Hispanic	-0.33	0.54	0.28	-0.76
White and other	--	--	--	--
E*Living Arrangement				
Living with parents	0.91	0.31	0.95	1.08
Living in supervised setting	--	--	--	--
Living independently	1.69	-1.57	1.09	1.55
E*Financial Management Skills				
Independent	-0.27	-0.38	-1.34	-0.93
Not independent	--	--	--	--
E*Receipt of Transfers				
SSI/SSDI	-1.40	1.36	0.07	-0.19
Other transfers only	0.30	-0.96	-1.62	-1.37
No transfers	--	--	--	--
E*Secondary Handicaps				
Secondary handicap	--	--	--	--
No secondary handicap	--	--	--	--
E*Cause of Retardation				
Organic	2.18	-2.30	0.00	0.25
Non-organic	--	--	--	--
E*Benefactor				
Benefactor	--	--	--	--
No benefactor	--	--	--	--
E*Work Experience in Two Years Prior to Enrollment				
Regular job lasting >3 months	-0.28	-0.12	-0.52	-1.40
Other job lasting >3 months	-0.06	2.01	0.59	2.11
Other	--	--	--	--
E*School Status at Referral				
Enrolled	0.37	0.23	-0.99	-0.36
Not enrolled	--	--	--	--

NOTE: All control variables are measured at baseline or referral. The variable definitions and their means are reported in Table A.1. **E*** indicates that a control variable has been interacted with **Experimental Status**. The t-statistics for the coefficient estimates are reported in Table A.9.

TABLE A.11

AREA UNEMPLOYMENT RATES DURING THE DEMONSTRATION AND EVALUATION PERIODS

(Percent of the Laborforce)

	Site[a]					United States[b]	
	Cincinnati	Los Angeles	New York	St. Paul	Tucson	Total	20 to 24 Years Olds
1981							
April	7.3	6.9	8.7	4.1	4.9	7.3	12.0
May	7.3	6.2	7.9	4.1	4.6	7.6	12.6
June	7.8	6.6	7.6	4.7	5.3	7.3	12.1
July	8.8	7.0	8.8	4.0	5.4	7.0	11.5
August	8.4	6.5	7.5	3.9	5.2	7.2	12.1
September	9.2	7.3	8.4	4.3	4.9	7.5	12.3
October	9.1	7.5	8.4	4.3	5.4	8.0	12.7
November	9.8	6.8	7.7	4.9	5.9	8.4	13.0
December	10.0	7.5	8.3	6.9	5.7	8.6	13.5
1982							
January	10.7	8.9	8.6	5.8	7.5	8.6	13.5
February	10.9	8.3	8.6	5.9	7.4	8.8	14.1
March	11.0	8.6	9.2	6.0	7.8	9.0	14.1
April	10.2	8.5	8.3	5.6	7.6	9.3	14.5
May	9.6	8.4	8.1	5.7	8.4	9.4	14.5
June	10.9	8.8	9.1	6.4	9.9	9.5	14.5
July	10.6	10.4	8.9	6.3	10.3	9.8	14.7
August	11.1	9.1	9.7	6.4	10.4	9.9	15.3
September	10.2	9.4	8.6	6.5	10.2	10.2	15.3
October	11.2	10.2	9.4	6.8	10.3	10.5	15.8
November	11.1	10.5	8.8	7.5	10.2	10.7	16.3
December	11.1	10.4	8.5	6.9	10.3	10.8	16.0
1983							
January	11.5	11.0	9.0	8.1	11.2	10.4	16.1
February	12.2	11.5	8.7	8.0	11.6	10.4	16.1
March	11.9	10.1	10.1	7.9	11.3	10.3	15.4
April	10.7	9.7	8.6	7.2	10.4	10.2	15.4
May	10.7	10.1	8.3	6.6	9.8	10.1	15.5
June	10.8	10.2	8.5	6.9	9.9	10.0	14.5
July	9.3	10.5	9.7	6.5	9.5	9.5	13.9
August	9.1	10.8	9.6	6.4	8.6	9.5	14.4
September	9.7	9.4	9.4	5.6	7.5	9.3	13.8
October	9.0	8.7	8.4	5.4	7.3	8.8	13.6
November	9.5	7.8	7.5	5.4	6.9	8.4	13.0
December	9.2	7.0	7.3	5.7	6.4	8.2	12.0
1984							
January	9.3	8.4	7.6	5.5	5.8	8.0	12.5
February	8.8	7.5	8.5	5.3	5.3	7.8	11.6
March	8.7	7.9	7.8	5.0	4.9	7.8	11.6
April	8.5	7.3	7.1	4.7	4.4	7.8	12.2
May	8.6	7.1	7.0	4.5	4.4	7.5	11.5
June	8.2	8.4	9.0	5.0	4.3	7.1	10.7
July	7.9	9.5	10.3	5.0	4.0	7.5	11.3
August	8.3	8.7	9.2	4.5	4.0	7.5	11.8
September	8.3	8.0	8.0	4.4	3.6	7.4	11.5

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[a] These data are from Labor Force and Unemployment by State and Selected Metropolitan Areas, Bureau of Labor Statistics, Washington, D.C.: Government Printing Office, (various issues.) The data are not seasonally adjusted.

[b] These data are seasonally adjusted figures from Employment and Earnings, Bureau of Labor Statistics. Washington, D.C.: U.S. Government Printing Office, (various issues.)

TABLE A.12

NUMBER OF PARTICIPANTS AND CONTROLS WITH VARIOUS CHARACTERISTICS USED TO DEFINE KEY SUBGROUPS

Characteristics at Baseline	Month 6		Month 15		Month 22	
	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Total	**145**	**138**	**204**	**198**	**206**	**189**
Site						
Cincinnati	29	29	39	44	44	34
Los Angeles	30	27	47	39	46	38
New York	35	34	49	47	44	50
St. Paul	18	18	24	26	26	25
Tucson	33	30	45	42	46	42
IQ Level						
Borderline	41	42	57	61	54	60
Mild	84	81	123	118	126	111
Moderate	20	15	24	19	26	18
Age						
Younger than 22	100	107	134	144	135	137
22 or older	45	31	70	54	71	52
Gender						
Male						
Female						
Race/Ethnicity						
Black	41	49	61	68	59	64
Hispanic	14	17	28	30	27	29
White and other	90	72	115	100	120	96
Living Arrangement						
Living with parents	113	122	160	171	161	161
Living in supervised setting	15	6	16	16	23	17
Living Independently	17	10	22	11	22	11
Financial Management Skills						
Independent	42	40	61	49	61	47
Not independent	103	98	143	149	145	142
Receipt of Transfers						
SSI/SSDI	47	45	68	67	67	66
Other transfers only	44	39	61	68	61	69
No transfers	54	54	75	63	78	54
Cause of Retardation						
Organic	23	23	37	28	38	29
Non-organic	122	115	167	170	168	160
Work Experience in Two Years Prior to Enrollment						
Regular job lasting ≥ 3 months	23	19	31	18	31	27
Other job lasting ≥ 3 months	52	50	74	66	71	64
Other	70	69	99	114	104	98
School Status at Referral						
Enrolled	36	50	53	72	52	70
Not enrolled	109	88	151	126	154	119
Total in Sample	283		402		395	

NOTE: These figures pertain to the samples used to estimate the employment results reported in Table IV.6. The sample sizes differed slightly for other subgroup analyses, due to different patterns of missing data for the outcome measure.

TABLE A.13

ESTIMATED PROGRAM IMPACTS ON THE PERCENT IN WORKSHOPS OR ACTIVITY CENTERS, BY KEY SUBGROUPS OF STETS PARTICIPANTS

Subgroups Defined by Characteristics at Baseline	Month 6			Month 15			Month 22		
	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact	Experimental Group Mean	Control Group Mean	Estimated Impact
Total Sample	7.8	14.5	-6.7*	6.4	15.6	-9.2**	7.9	19.6	-11.7**
Site									
Cincinatti	56.9	4.4	13.2	-2.0	10.0	-12.0*	5.5	13.6	-8.1
Los Angeles	-2.1	22.6	-24.7**	9.2	12.6	-3.4	4.1	12.8	-8.7
New York	-7.8	16.8	-14.6*	2.6	18.0	-15.4**	6.2	18.6	-12.4*
St. Paul	23.1	5.4	17.7	17.3	25.0	-7.7	12.9	48.1	-35.2**
Tucson	5.4	18.8	-13.4	9.4	15.8	-6.4	12.2	15.5	-3.3
IQ Level									
Borderline	9.2	11.1	-2.7	9.4	14.2	-4.8	9.2	10.9	-1.7
Mild	13.0	15.2	-5.6	4.3	12.8	-8.5**	6.2	23.1	-16.9
Moderate	0.0[b]	21.8	-21.8*	10.3	35.3	-25.0**	9.3	22.9	-13.6
Age									
Younger than 22	6.4	11.1	-5.5	5.7	11.4	-5.7	7.0	13.9	-6.9*
22 or older	13.7	23.8	-10.1	8.2	25.2	-17.0**	9.7	32.0	-22.3**
Gender									
Male	8.1	14.4	-6.3	9.0	17.7	-8.7**	9.8	22.4	-12.6**
Female	7.4	14.6	-7.2	2.8	12.7	-9.9**	5.3	15.9	-10.6**
Race/Ethnicity									
Black	7.4	13.4	-6.0	12.1	16.2	-4.1	9.2	17.8	-8.6
Hispanic	19.6	2.8	16.8	2.9	15.6	-12.7	8.4	12.1	-3.7
White and other	5.7	17.4	-11.7**	4.0	15.3	-11.3**	7.0	22.5	-15.5**
Living Arrangement									
Living with parents	8.7	16.1	-7.4*	6.2	16.0	-9.8**	7.9	21.0	-13.1**
Living in supervised setting	9.6	19.3	-9.7	14.5	24.7	-10.2	13.8	24.6	-10.8
Living Independently	5.3	0.0[a]	5.3	-0.8	0.8	-1.5	-0.6	0.0[a]	0.6
Financial Management Skills									
Independent	6.9	8.7	-1.8	1.6	11.3	-9.7	2.5	13.5	-11.0*
Not independent	8.2	16.8	-8.6*	8.2	17.2	-9.0**	9.8	21.8	-12.0**
Receipt of Transfers									
SSI/SSDI	18.2	15.1	3.1	12.1	16.1	-4.0	12.1	22.8	-10.7*
Other transfers only	1.6	19.4	-17.8**	2.8	15.7	-12.9**	7.3	14.5	-7.2
No transfers	3.4	10.1	-6.7	4.4	15.1	-10.7**	4.0	21.1	-17.1**
Cause of Retardation									
Organic	8.4	28.4	-20.0**	10.3	27.9	-17.6**	16.5	23.3	-6.8
Non-organic	7.6	11.7	-4.1	5.6	13.1	-7.5**	6.1	18.8	-12.7**
Work Experience in Two Years Prior to Enrollment									
Regular job lasting >3 months	-0.4	10.0	-10.4	0.9	15.7	-14.7*	0.0[b]	5.4	-5.4
Other job lasting >3 months	7.5	14.4	-6.9	9.5	21.5	-12.0**	11.6	26.4	-14.8**
Other	10.3	15.9	-5.5	5.9	11.7	-5.8	7.8	19.2	-11.4**
School Status at Referral									
Enrolled	1.3	12.8	-11.5	3.3	20.8	-17.5**	4.1	16.7	-12.6**
Not enrolled	10.6	15.2	-4.6	7.9	13.3	-5.4	9.6	20.9	-11.3**
Number in Sample			287			414			402

NOTE: These results were estimated through ordinary least squares techniques. In addition to the control variables that are included in the models which underlie the overall net impact estimates report in Table IV.4, these models include variables that interacted the treatment variable with the subgroup variables.

[a] The control group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

[b] The experimental group mean value was actually calculated to be slightly negative because of the imprecision of OLS estimation with a binary outcome variable.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two tailed test.

TABLE A.14

ESTIMATED IMPACTS ON THE DISTRIBUTION OF TOTAL WEEKLY INCOME

	Month 6			Month 15			Month 22		
	Experimental Group	Control Group	Experimental Control Difference	Experimental Group	Control Group	Experimental Control Difference	Experimental Group	Control Group	Experimental Control Difference
Percentage Distribution									
$0	19.01	27.07	-8.06	18.78	20.00	-1.22	20.59	19.58	1.01
$1-$60	17.61	39.10	-21.49	32.99	36.41	-3.42	26.47	32.28	-5.81
$61-$100	37.32	14.29	23.03	16.75	22.05	-5.30	21.57	28.04	-6.47
More than $100	26.06	19.55	6.51	31.47	21.54	9.93	31.37	20.11	11.26
Average	$73.78	$49.63	$24.15	$70.62	$58.24	$12.38	$72.52	$60.69	11.83
Median	$80.21	$35.00	$45.21	$59.51	$50.46	$9.05	$62.80	$55.76	7.04
Number in Sample	142	133	275	197	195	392	204	189	393

NOTE: These data are not regression-adjusted. Thus, the mean values (averages) differ slightly from the figures reported in Table VII.5.

TABLE A.15

AVERAGE OPERATING EXPENDITURES FOR TRANSITIONAL AND SUPPORTED EMPLOYMENT PROGRAMS
(1982 dollars)

Program	Average Expenditure per Client Year	Average Expenditure per Participant
Alternative Programs for Mentally Retarded Young Adults		
Special Education, EMR (Kakalik et al., 1981)	5,617	n.a.
Special Education, TMR (Kakalik et al., 1981)	8,168	n.a.
Sheltered Workshops (U.S. Department of Labor, 1977)	5,920	n.a.
Work Activity Centers (U.S. Department of Labor, 1977)	2,525	n.a.
STETS Program (Riccio and Price, 1984)		
Cincinnati	10,311	8,420
Los Angeles	8,743	7,286
New York	11,467	9,651
St. Paul	5,411	4,283
Tucson	6,724	7,060
Average for all participants	8,715	7,553
Virginia Commonwealth University: Rehabilitation Research and Training Center		
Transitional Employment (Hill et al., 1985b)	7,119	3,286
Supported Employment (Hill et al., 1985b)	2,404	n.a.
Project Employability (Hill at al., 1985a)	n.a.	6,264[a]
Bay State Skills Corporation (Bailis, 1984)[b]	9,280	3,280
University of Washington Food Service Program (Moss, 1980)	10,771	9,580

NOTE: While an attempt was made to make the cost estimates as consistent as possible, differences still exist. For example, the costs for STETS and the Bay State Skills Corporation include some wage payments to participants, while the Virginia Commonwealth estimates do not include any payments to clients. Costs have been inflated to 1984 dollars by using the change in the implicit price deflator for gross national product.

n.a. means that data are unavailable.

[a] Because Project Employability combines transitional and supported employment, the total cost per participant will depend on the length of stay. The cost estimates cover 70 months of operation, although most persons had not been enrolled for that long. Thus, costs per participant will continue to rise as supported employment services continue to be provided.

[b] The figures from Bay State Skills Corporation reflect the experiences of 17 vendors who enrolled a total of 306 clients during fiscal years 1982 and 1983. Average costs per participant for these vendors ranged from about $5,500 to $1,700.

APPENDIX B

SURVEY DESIGN AND IMPLEMENTATION

The STETS evaluation required data on the activities and experiences of experimentals and controls from the time of their contact with the program and randomization into the research sample to a point 22 months later. These experiences included labor-market activities, participation in training and schooling programs, receipt of transfer payments, use of support services, and other activities pertaining to self-sufficiency. Data were also required on important demographic and personal characteristics. The data for the evaluation had to be collected in a standardized manner for all sample members (whether in the experimental or control group) over time at appropriate intervals to capture the effects of the intervention and key program events, as well as consistently across the five demonstration sites. This appendix describes in detail the data collection design, and reviews several important methodological and fielding issues addressed in this design. It also provides information on the results of the data collection effort.

A. DESIGN OF THE STETS DATA COLLECTION STRATEGY

The strategy designed by MPR to collect the data for the impact and benefit-cost analyses consisted of many components, and was developed from our own and others' experiences. In this section, we document the background of that strategy, and briefly describe the key components.

1. Background to the Development of the Data Collection Strategy

The data-collection approach proposed by MPR to evaluate the STETS demonstration was the result of an extensive investigation and evaluation of various methods that have been used in previous studies on mentally retarded persons and on employment and training programs in general. In order to develop a strategy that was likely to yield the best data in the most cost-effective manner, MPR conducted the following:

- A review of literature and available published information from other studies[1]

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[1] Bibliographies of the important references reviewed in conjunction with the design activities can be found in Burghardt, Corson, and Maynard (1980) and in the pilot study report (Bloomenthal et al., 1982).

- An extensive discussion with expert consultants who had collected data from samples of mentally retarded persons in previous studies

- A review of MPR's and other groups' experiences in interviewing populations with similar characteristics

In other evaluations, similar data collection requirements have usually been met by having trained research interviewers administer structured interviews to sample members. However, the STETS sample members' expected lower-than-average levels of functioning, cognitive abilities, and communication skills raised serious concerns about the quality of self-reported data. Previous research on this target population relied on data from various sources and on a variety of data collection techniques. Probably the most common source of data has been "significant others" (for example, counselors, job supervisors, and parents and guardians) as informants or proxies (see Rusch and Schutz, 1980; Hunt and Zimmerman, 1969; Bogen and Aanes, 1975; Lambert and Nicoll, 1976; Eyman et al., 1979; and Abramowitz, 1980). In many instances, parents or caretakers were expected to articulate the experiences or capabilities of the mentally retarded persons who were not interviewed themselves. To date, self-reported interviews with mentally retarded persons have been used primarily to provide anecdotal details rather than information on major variables for statistical analysis (e.g., Wyngaarden, 1981). However, there is evidence that individuals who are mildly or moderately retarded are willing and able to provide some portion of the data necessary for evaluation research through in-person interviews (see, for example, Weinglass, 1980; Richardson, 1979; Gollay et al., 1978; Sigelman et al., 1981a; Birenbaum and Re, 1979; and Brolin, 1972).

As a result of our preliminary review of previous data collection efforts, MPR formulated a basic approach, relying primarily on self-reported interview data from the mentally retarded sample members. The data required for the STETS evaluation were collected through an integrated system of data collection efforts, including:

- Interviews conducted with the mentally retarded sample members and, as necessary, with proxy respondents when sample members were unable to provide key data items
- Corroborating information provided by community service agencies with which the sample had contact and which were mentioned during the interviews
- Background information on sample members collected by STETS project staff as part of the intake process
- Program participation data on all experimental group members while enrolled in the STETS projects

The sample member (and proxy) interviews were conducted at four key points during the demonstration:

1. Immediately after random assignment into the sample (the baseline interview)
2. At a point when many experimental group members were still actively participating in the STETS project (the 6-month interview, conducted immediately after 6 months had elapsed from an individual's random assignment into the sample)[1]
3. At a point when members of the experimental group were no longer receiving STETS services (the 15-month interview)
4. At a point well beyond the end point at which individuals stopped receiving demonstration program services (the 22-month interview)

Through our review of previous efforts, we identified several critical problem areas in the design of self-reported data collection strategies with mentally retarded persons. For example, researchers have identified a consistent pattern of acquiescence among mentally retarded respondents in interview settings (Gerjony and Winters, 1966; Rosen et al., 1977; and Sigelman et al., 1981b). This pattern is not surprising given general population survey findings which have suggested that acquiescence ("yea-saying") is more common among both children and less educated adults (Lenski et al., 1960; Wells, 1963; and Rothenberg, 1969). However, this problem, which is likely to affect the reliability and validity of self-reported data, has been found to be most serious among lower IQ samples and is only somewhat less problematic among those with IQ levels that are characteristic of the STETS sample (Sigelman et al., 1980). Interviewer behavior, as well as question wording and response formats, have also been found to affect the quality of survey data from mentally retarded respondents. As an example, biased responses due to "test anxiety" and a heightened desire to please the interviewer are likely scenarios with this population (Sigelman et al., 1983). The design of the STETS interview instruments and procedures took into account the sample's expected levels of cognitive and communication abilities and interaction skills.

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[1] A two-thirds sample for the 6-month follow-up survey was determined to be sufficiently large to detect impacts on sample member activities while enrolled in the STETS project, since activities of experimental group members at that time would be determined largely by participation in STETS.

The following sections review the requirements for each data collection instrument developed for the evaluation, as well as the issues addressed in their design.

2. Sample Member and Proxy Interview Instruments

The baseline and follow-up interviews collected point-in-time data on the following:

- Current employment, job training, and schooling activities
- Current involvement in life-skills training, organized recreational activities, counseling, and transportation assistance programs
- Current receipt of transfer payments or benefits, including SSI, SSDI, general assistance or welfare, Medicaid or Medicare, food stamps, and other government or private financial assistance
- Current living arrangements and residential services

- Current participation in money-handling activities including shopping, bill-paying, and banking[1]

The sample member interview was developed by MPR staff, with substantial input from our expert consultants. A number of versions were pretested with mentally retarded young adults, and although all of the pretest interviewees had IQ scores in the range of interest (40-80) we concentrated on lower-functioning individuals so as to assess using the questionnaire with more problematic respondents. By the time the pilot instrument was fielded, over 30 pretest interviews had been conducted with a variety of mentally retarded individuals in a number of living and work arrangements. The proxy interview was designed to be a close replica of the sample member interview. Questions and formats were modified only when a question was inappropriate for a nonretarded respondent or to accommodate mixed-mode (telephone and/or in-person) administration. Ten pretest proxy interviews were conducted.

Measurement design is especially critical to the success of a self-reported interview strategy with a mentally retarded population. For example, past experience suggests that questions involving recall are likely to present problems for this group. Detailed reports of dates and other aspects of past experiences are believed to be especially unreliable. Therefore, the STETS interview instruments asked for reports of current activities only. As described earlier, there is also evidence of a consistent pattern of acquiescence among mentally retarded respondents in interview settings, although this pattern is less serious among IQ populations that are characteristic of this sample. The approach taken in the STETS interview was to follow "yes" answers with questions on the details of the activity or experience, to ensure that the initial response was not due to acquiescence.

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[1] The baseline interview also contained information on the work history and occupation of those with whom the sample member had lived while growing up.

Quantitative concepts such as those involving time and money are especially difficult for mentally retarded persons. Mentally retarded individuals' knowledge about one particular area of interest to the STETS evaluation--their financial situation (sources and amounts of income, expenses, and assets)--also varies greatly according to both their level of independence and their cognitive abilities. There may be fairly large gaps in the knowledge of some sample members in terms of the details of their financial status, depending on whether they handle their own finances. In the STETS interview instrument, questions on earnings and the receipt of transfer payments, as well as on other quantitative concepts, were broken down into simpler subquestions.[1] For example, earnings on a job was determined by asking for the rate of pay, the frequency of receiving pay, and the usual (or last) amount received. If the rate of pay was unknown, the other questions were used to construct it.

Both interviewer behavior and question wording and response formats can affect the quality of survey data. One major concern was the number and directiveness of probes. The mentally retarded are likely to be unsure of their answers and might initially respond "don't know" to many questions, both factual and attitudinal. However, excessive probing may provoke biased responses due to "test anxiety" and a heightened desire to please the interviewer. Therefore, the STETS interview instrument specified the exact number and type of probes to be used by the interviewer on items thought to be particularly likely to require probing.

3. Agency Service Interview and Coding Form

During the interviews with primary and proxy respondents, the following entities were identified and assigned a unique identifying code: community service providers, employers, and residential service agencies. Interviewers also obtained from the respondents sufficient information to contact the organization, and then attempted brief telephone interviews with knowledgeable informants at each organization. These interview instruments were designed to determine the types of clients served and the mix of services provided if the organization was in fact a service agency. Private employers and other organizations which did not provide services were asked an abbreviated set of questions. Sample members who had named the organizations were not identified, and no attempt was made to collect individual-level service data. Agency-level data were used to corroborate sample member reports on services received, including the type of residential arrangements, schooling, and employment training. These data were also used to estimate the cost of services received by the sample, either as an alternative to or in addition to STETS, for application in the benefit-cost analysis.

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[1] These questions were among those for which missing or inconsistent responses indicated the need for a proxy respondent.

4. Application/Enrollment Form

As part of the intake process, STETS project staff completed application/enrollment forms for all sample members. The application/enrollment form summarized information collected from a number of sources--the applicant, parents or guardians, referral agency staff or records, and records or reports from other agencies. It contained information certifying the applicant's eligibility for the STETS demonstration (date of birth, IQ score and documentation, recent work history, and secondary handicaps), as well as other background information, including living arrangements, parental background, and history of schooling, training, and employment. The information from the application/enrollment form provided data pertaining to two important topics: (1) the baseline experiences and characteristics of the sample prior to the receipt of any STETS services, and (2) the past education and employment services received by sample members. Both topics were critical to the analysis. Data pertaining to both could not be gathered reliably in the first (baseline) survey, because the time required for assignment and contact attempts meant that baseline interview data from the sample members were obtained at an average of approximately thirty days after random assignment, when most experimental group members would have begun to participate in the STETS program. Moreover, such data could not be sought retrospectively in the first interview, because of the limited ability of the sample to report details of their past experiences accurately (the interview asked about current activities only).

The staff of MDRC and MPR met several times in the process of developing the application/enrollment form and drew upon an early review of draft forms by STETS operators and some of their referral agencies. The form underwent several modifications while used during the early months of program intake. The initial instructions to the sites were to complete the entire form regardless of the data source; later, the emphasis shifted to completing most of the data items from records.

5. STETS Participation Data

The application/enrollment form for each experimental group member initiated an entry for that individual in the Management Information System (MIS) maintained by MDRC for the STETS projects. During the period of the participation of experimental group members in the STETS demonstration, information on each individual was provided on a monthly basis to the MIS database. This information included the individual's current status in the project, placement data on training and permanent jobs, reasons for changes in program status (from the Monthly Status Change Form), the number of days actively involved in STETS, and the hours scheduled and actually attended in various types of demonstration activities (from the Monthly Activity Form). These data were used in the impact and benefit-cost analyses to determine the length of program participation for individuals and the level of STETS services provided to them.

B. PILOT STUDY

Following the design period, the data collection for the STETS evaluation proceeded in two phases. Between November 1981 and January 1982, a pilot study, including interviews with sample members and proxy respondents, was conducted with treatment and control group members in three sites. The pilot study results were used to modify the instruments and procedures. The second phase of the data collection began in April 1982, when fielding began in all five sites, and continued until October 1984, when the last follow-up interviews were conducted. This section briefly reviews the results from the pilot study; further details on the procedures and findings of this study are available in a separate report (Bloomenthal et al., 1982).

1. Study Overview

A pilot study was undertaken to inform final design decisions about the best source(s) of data for the evaluation, given the uncertainty about the quality of self-reported interview data from mentally retarded sample members. The pilot study entailed conducting data-collection activities with the research sample which was enrolled between November 1, 1981 and January 31, 1982 in Cincinnati, New York, and Tucson. The pilot-phase design called for interview attempts with all research sample members and an identified proxy for each respondent. Application/enrollment forms for each sample member were received, and the data were entered for data comparisons. Data from a total of 104 sample members were included in the pilot study analysis. The study also investigated the availability and quality of data from official records.

The pilot study confirmed the ability of most of the STETS sample to respond to research interviews and generally to provide complete and accurate data on themselves. Records and proxy respondents were not found to be superior sources, in terms of either completeness or data quality. The key findings of the study are summarized in the following sections.

2. Sample Completeness

High response rates with both sample members (95 percent) and proxy respondents (99 percent) indicated that the interview strategy could provide baseline and follow-up data on virtually all sample members. The application/enrollment form also provided a high degree of sample completeness, but was available only for certain baseline data items.

3. Data Completeness

Little or no missing data occurred for many of the variables in the pilot study data, including the education and training variables--both current (from the interview) and prebaseline (from the application/enrollment form) living arrangement and family composition (from all three sources), and other living-skills activities (e.g., independence in money-handling from the interviews). Other types of variables had greater levels of missing data, regardless of the source. Transfer-program use was the most striking case. Some aspects of labor-market performance, particularly earnings, also suffered from substantial missing data from both the sample member and the proxy interviews.

The missing interview data found during the pilot study followed the patterns that were expected from a review of the available literature, the experience of consultants, and pretest experiences. Key areas were those that involved money, particularly the amounts of earnings and the receipt and amount of transfer payments. In the area of transfers (both cash and in-kind), patterns of nonresponse by sample members led us to believe that a "don't know" response might indicate a reluctance to say "no" when the question seemed ambiguous. From these patterns, we were able to design an appropriate rule for using proxy interviews which significantly decreased the amount of missing data.

The missing data encountered on the application/enrollment form were due to a variety of problems, and there were significantly more missing data on the form than in the interviews. However, the form did provide adequate completeness on some key data items (e.g., IQ) that were not available from other sources.

4. Data Consistency

An analysis of data consistency across sources, together with the analysis of completeness, enabled us to draw inferences about quality. Generally, the consistency between sample member and proxy pilot study data was quite high. Where reporting differences did appear, there were indications that any errors underlying the inconsistencies were as likely to come from proxy respondents as from sample members.

C. FIELD PROCEDURES

This section describes the procedures used to implement the data collection design. These field procedures were initially used during the pilot phase, and were then modified and extended to the second phase of data collection. The procedures discussed here include interviewer recruitment and training, interviewing, supervision, and quality control.

1. Interviewer Recruitment and Training

The interviewing staff was critical to the success of the STETS evaluation. Field interviewers were responsible for implementing the data collection design through interactions with the mentally retarded sample members and their parents, guardians, and other caregivers, with the STETS project staff, and with directors of the many community agencies from which the sample received services. They had to maintain detailed confidential records to help locate sample members for the follow-up interviews. Because of the small sample size, only one interviewer was hired in most sites, and that person had to be able to carry out all field data collection tasks independently, without face-to-face daily supervision. The importance of field staff to the evaluation dictated very careful interviewer recruitment and training efforts.

Recruitment. Applicants for interviewer positions were recruited primarily through classified advertisements in the major newspaper in each of the demonstration sites or through recommendations from the STETS operators in each site. Applicants who responded to our newspaper advertisements and who had relevant experience, both in working with the mentally retarded or similar populations and in performing research interviewing, were contacted by telephone. An outline that was followed during this telephone conversation gathered more details on the quality and extent of the interviewer applicants' experience and assessed their willingness to undertake the work. The following types of experiences and attitudes were assessed:

- A willingness to be a data collector without being able to offer advice, referrals, or services

- An interest in the study content and an understanding of the general research objectives
- Experience in setting up and maintaining files and records and with regular reporting/monitoring procedures
- Experience in arranging a schedule of appointments by telephone and in locating difficult-to-locate persons
- Having a home office already established, with work and storage space as well as a telephone

Applicants were also asked about their current employment or other commitments and whether they had regular use of an automobile. These telephone screening interviews were reviewed, and the best set of applicants were contacted for in-person interviews.

Senior survey staff traveled to the sites and conducted in-depth interviews with the selected applicants. In-person recruitment interviews were scheduled for two days--the first day involving formal interviews with applicants, and the second day involving visits to the homes of the top two to four candidates. The formal interviews were designed to provide a more detailed follow-up on the relevant experience and background of the applicants by questioning them about areas of concern that had been identified in the telephone screening. The applicant's general style was also crucial, since his or her role would involve contact with the STETS program and other local agencies, with parents and guardians, and, of course, with the mentally retarded young adults themselves. Applicants were also asked to conduct a brief mock interview with an MPR staff member who acted as a respondent.

The home visit allowed the recruiter to talk with and observe the applicant in a more relaxed setting, as well as to answer any additional questions he or she might have about the job, to obtain more details on potential issues of concern (flexibility, travel time, other commitments, etc.), and to look over the available office space. Extensive reference checks with recent past employers were also part of the final decision process.

The STETS Interviewers. Four pilot study interviewers were hired--one each in Cincinnati and Tucson and two in New York. One New York interviewer was a woman with an educational background in clinical psychology, who had experience in one-on-one tutoring and had worked in a group home with mentally retarded young people. The other New York interviewer was a man who had interviewing experience on an MPR study of the impaired elderly. The woman hired in Tucson was a tutor and counselor with the mentally retarded in the same agency which was conducting the STETS program. The Cincinnati interviewer was a woman with a background in volunteer work and paid employment with the mentally retarded, and who had helped compile a local directory of services for this group.

After the pilot study, the female New York interviewer was retained for the full study, and the Cincinnati interviewer was replaced by a woman who had worked for the local STETS host agency prior to taking maternity leave. She had had extensive experience in counseling and training mentally retarded persons. Interviewers were also hired at that time in the Los Angeles and St. Paul sites. Two women were recruited in Los Angeles to cover the large catchment area of that STETS project. One of the Los Angeles interviewers was a woman with teaching experience and who was a MPR interviewer. The other woman was an interviewer with personal experience in working with mentally retarded young adults. The St. Paul interviewer was a woman with experience in both counseling and research interviewing.

There was virtually no interviewer attrition after the pilot study. One Los Angeles interviewer was laid off in the summer of 1982 due to the lack of work, and the remaining interviewer continued with the project to its completion. The New York interviewer left the project in February 1983 to take a full-time job as a counselor in a community residential program for mentally ill clients. She was replaced by an experienced interviewer, a man who had worked with MPR on a number of youth employment studies. This interviewer underwent thorough individualized training, assisted by the outgoing New York interviewer. He and the other interviewers continued on the project until its completion in the fall of 1984.

Training. The role of the interviewer involved a complex set of tasks, as well as interaction with different individuals and agencies in the process of completing a single interview assignment. The training sessions were necessarily lengthy and intensive, involving practice sessions and feedback by MPR staff, with special tutoring as necessary.

Training was conducted during four-and-a-half-day sessions (December 7-11, 1981, for the pilot study, and April 12-16, 1982, for the ongoing study) held at the MPR offices in Princeton. Two manuals were prepared for these sessions, covering the full range of field issues and activities--Interviewers' Procedures Manual and Instrument Training Manual: Primary and Proxy Instruments and Agency Log.

These manuals served as the basis for the training sessions, providing detailed information on all aspects of field procedures and questionnaire usage. A considerable portion of the training was devoted to the practical use of the various forms and instruments. MPR survey staff held several round-table and one-on-one mock interviews with the trainees, and observed and commented on all aspects of questionnaire administration and field procedures. Interviewers who had pilot study field experience assisted in the training, sharing their experiences and demonstrating effective techniques for contact attempts, interviewing, and record keeping.

One of the most useful activities during training was the interviews conducted by the interviewer trainees with local mentally retarded young adults, most of whom were at the lower end of the STETS eligible range in terms of IQ and functional ability. These "real-world" practice interviews gave the interviewer trainees confidence in their ability to handle field situations with mentally retarded respondents before they were in the field. Moreover, MPR survey and research staff who observed the interviews had the opportunity to provide better assessments of the strengths and weaknesses of the individual interviewers. The round-table debriefing held afterward was a time to share problems, discuss possible solutions, and provide feedback on interviewer performance.

Mid-Project Interviewer Conference. On January 24-25, 1983, an interviewer conference was held in Princeton to review changes in project schedules and procedures. This conference was attended by all the survey staff, with the exception of the Tucson interviewer, who participated, as possible, by telephone. Four main topics were discussed at the conference:

1. Administering the interviews and further detailed instructions, based on interviewer experiences and questions
2. Contacting respondents for the follow-up interviews, particularly the one-third of the sample who were not assigned the 6-month follow-up
3. Conducting the agency services interviews and coding the services log
4. Administrative procedures and issues pertaining to interviewing assignments and pay schedules

The conference provided an excellent forum for research, survey, and field staff to review the goals of the STETS evaluation, discuss data quality issues, and resolve field problems. A member of the MDRC staff also attended the conference.

2. Interviewing

Interviewing activities in the field for the baseline pilot study began on December 14, 1981, and ended on January 31, 1982. The second phase of interviewing began on April 19, 1982, after the final data collection design had been approved by MDRC, additional interviewers were recruited, and interviewer training had been completed. The field period ended on October 31, 1984.

Assignments and Contact Attempts. During the fielding of the baseline interviews, weekly assignments (when intake warranted them) were sent to interviewers from the logs kept of the applicants who had been randomly assigned at each site. Interviewers were sent the name and identification number of each new sample member. Interviewers were expected to pick up from the STETS program the consent materials and application/enrollment forms that were necessary to begin scheduling interviews.

At the time of application to the STETS projects, project staff explained the conditions of participation in the demonstration and the associated evaluation--in particular, random assignment and periodic research interviews. Before applicants could be enrolled in the research sample and, if experimental group members, provided with demonstration services, their written consent had to be obtained and co-signed by a parent or guardian if necessary. The baseline interview could not be conducted until the research interviewer had obtained this consent form from the STETS project. No further written consent for the follow-up interviews was obtained from the primary sample members. However, at the time each interview was conducted, the interviewers answered any questions and explained the voluntary nature of the interview and the confidentiality of information obtained during the interview.

Information from the application/enrollment form was used to prepare advance letters and other material for interviewing contacts. These letters were followed by telephone calls to the primary respondents to arrange an appointment for an in-person interview. At that time, the interviewer also spoke with the parent, guardian, houseparent, or other responsible person if the primary respondent was not living independently. If there was no telephone number on the application/enrollment form or if the contact information was no longer valid, the interviewer made personal visits to the home and/or initiated search procedures until the primary respondent could be located or a final noncompletion status assigned. Interviewers kept detailed records on all contact attempts for every sample member during both baseline and follow-up fielding.

The sample member was interviewed at the scheduled time in the home if possible or someplace else where the sample member would feel at ease. Upon completion of the interview, the sample member was given a $5 cash respondent payment. These payments were well received and seemed to contribute to the respondents' willingness to be interviewed. Interviewers obtained receipts for the respondent payments and submitted these receipts along with their other interviewing expenses. Proxy respondents did not receive any payments.

During the follow-up interviewing, assignment sheets which listed the name and identification number of the sample members who were eligible for an interview during the upcoming month were sent to the interviewers at the end of the preceding month.[1] Advance letters, telephone calls, and in-person attempts were used to contact and interview the follow-up sample. During the follow-up interviewing, interviewers were allowed to use their own judgment in deciding whether to send advance letters to parents when the sample member did not live at home.

Identifying and Interviewing Proxy Respondents. Critical items on the sample member interview were used to determine the necessity of conducting a proxy interview. These included items which identified the sample member's major activity (employment, training, or schooling), hours and earnings of any employment-related activities, and the receipt or amount of cash benefits from government transfer programs or other sources. Based on specific instructions in the sample member interview, interviewers noted cases in which the sample member was unable to provide the required information, where the information was inconsistent (specifically, when reported SSI benefit amounts exceeded the maximum possible in the state of residence), and when the sample member appeared generally confused or was unintelligible.[2] In these cases, an appropriate proxy respondent was selected from those who were named during the sample member interview as providing significant services or support. The proxy respondent was selected in the following order of priority:

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[1] Sample members were eligible for the follow-up interviews only if their completed baseline interview had been received in Princeton. The 6-month follow-up interview was attempted only for a random two-thirds of the full sample, indicated at the time of randomization.

[2] A set of questions was included as the first module in the interview to determine the sample member's name, address, telephone number, and age. Besides providing a non-threatening introduction to the interview, the original intent of this module had been to identify respondents who could not provide this basic information, as a way to screen out those who were unable to complete the interview. However, most sample members could answer all these items correctly, thus making it useless as an early screen for those who would need proxy respondents. IQ score was also found to be an inadequate predictor of the necessity for a proxy interview.

1. A live-in parent or relative who gave help with financial management
2. Any other person who gave help with financial management
3. A live-in parent or relative, when no help with financial management was received
4. A social worker or caseworker
5. Someone whom the sample member indicated was knowledgeable, when no other criteria were met

The proxy respondent was interviewed immediately following the sample member interview if possible; if not, further contacts were made until an interview could be scheduled and completed. Additional letters were sent to proxies who had not been contacted during the initial contact process. After the pilot study (in which in-person interviews were required), interviewers could conduct the proxy interview over the telephone, if necessary.

Field Editing and Document Transmittal. After the interviews were completed, interviewers edited all the instruments and forms. Marginal notes were to be added as necessary to explain special circumstances or to provide details on ambiguous situations. Agency names mentioned during the sample member or proxy interview were entered onto an agency log, and code numbers were assigned and transferred to the interview documents. Agencies were contacted and asked to describe their services in order to complete the agency log form.

Interviewers maintained files for each sample member in their site. These files included contact worksheets which contained information that would be useful in later contact attempts. Such information included the names, addresses, and telephone numbers of friends or relatives who were likely to know where the sample member could be located, and the agencies or organizations in which he or she had been active. These notes also included information on any problems encountered during the interview administration, such as very protective parents, speech impediments, or emotional upsets.

Documents were sent to MPR in Princeton in two separate packets--one for the confidential material (application/enrollment form, signed consent form to participate in the research, contact sheet, and signed release to interview the proxy), and another for the completed interview instruments themselves. Interviewers and MPR staff kept independent records of assignments, completions, and mailings, which were reviewed and reconciled weekly. Interviewers also reported their time and expenses on a weekly basis.

3. Supervision and Quality Control

Once received in Princeton, interview documents (interview instruments, contact sheets, release forms, and agency logs) were logged in and edited by an experienced quality control clerk. Interviewers were trained to make extensive marginal notes on any circumstances that would affect how responses were coded during the interview. The quality control clerk carefully reviewed all such marginal notes when evaluating the appropriateness of the coded responses. Items on the documents for which responses were missing, ambiguous, or contradictory to other responses were flagged, and these issues were discussed with the interviewer during the next telephone call. Issues that could not immediately be resolved by the interviewer were assigned for a call-back by the interviewer to the respondent. As necessary, memoranda were circulated to the quality control and interviewing staff who were responsible for reviewing recent policy decisions that affected their work. These memoranda often discussed how to handle unusual situations encountered during interviewing or brought to light during quality control editing.

The quality control clerk conducted verification interviews with a random subsample of completed sample member and proxy interviews. The rate of verification interviews assigned was greatest for new interviewers during the first several weeks of work, and tapered off to a less frequent but still regular schedule thereafter. Items on the verification interview confirmed that the interview had been conducted, asked some basic factual information about the sample member (which was compared with interview data), ascertained whether there had been any problems with the interviewer's conduct, and verified that the $5 respondent payment had been made. A total of 254 verification interviews were completed throughout the entire study period. During the pilot field period, verification interviews uncovered problems with two interviewers' work, which were then rectified at that time: the interviewers were terminated from the project. No problems of any kind were encountered either with the work of the remaining interviewers or with the work of the interviewers hired later in the project.

Interviewers had a regular weekly reporting schedule with the MPR survey manager. During these telephone reports, assignment logs were updated with new final statuses. Reports of interim statuses and mailings were also made on a case-by-case basis. Additional telephone calls, initiated either by the interviewer or by MPR survey staff, were made to clarify contact or interviewing situations, to review changes in documents or procedures, and to resolve any discrepancies or errors in the interviews. These calls were made very frequently at the beginning of the field period, as interviewers confronted new situations and as MPR project staff made necessary modifications to procedures based on unanticipated circumstances. As fielding proceeded, the calls were made less frequently.

The weekly telephone reports from the field formed the basis for the field-status reports monitored by MPR project staff and provided regularly to MDRC. The receipt of materials in the Princeton office and the progress of these materials through quality control and data entry were also recorded.

Periodic site visits with each interviewer by MPR survey and research staff were conducted throughout the field period. During these visits, interviewer records and files were reviewed, interviewing or record-keeping problems or concerns were discussed, and an interview with a sample member was observed.

The STETS program operators were responsible for completing the MIS application/enrollment forms. Completed application/enrollment forms were sent to MDRC for quality control and site call-backs, where indicated. After problems were resolved, the forms were sent to MPR for additional quality control, coding for selected research questions, and data entry.

C. INTERVIEWING RESULTS

Overall, the data collection strategy using interviews with sample member and proxies was very successful, achieving both high completion rates and data of good quality. This section documents the interviewing results for each interview wave by type of respondent (sample member and proxy), site, and research status (experimental and control). In addition to the final status of each interview attempt, we present information on the completed interviews--elapsed time between assignment and completion, length of interview, location of interview, and other details of the interview process. The completeness of the resulting data set is also discussed.

1. Interviews with Sample Members

Tables B.1 through B.4 present the final statuses and response rates for interview assignments with the mentally retarded sample members at each wave. Response rate is defined as the number of completed interviews divided by the total sample assigned less those ineligible to be interviewed because they were incarcerated or deceased, or had moved out of the study area. These interviewing results show consistently high completion rates of interviews across sites and between the research statuses. At baseline, 455 interviews were completed, for an overall response rate of 97.6 percent (see Table B.1). There was only a small difference (3 percentage points) in the overall response rate between the experimental group and the control group (99.1 versus 96.1 percent), although these differences varied by site. In all cases, the response rate was greater for the experimental group. There were no substantial differences in the overall response rates among the sites, the largest being only 5 percentage points between Los Angeles and St. Paul.

TABLE B.1

SAMPLE MEMBER BASELINE INTERVIEW
FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS
(Percent)

		Final Status								
	Total Assigned	Complete/ Partial Complete	Sample Member Refused	Parent/ Guardian Refused	Non-English-Speaking	Incar-cerated	Moved Out of Area	Deceased	Unable to Locate or Contact	Response Rate[a]
Cincinnati										
Experimental	48	97.9	0	0	0	0	0	0	2.1	97.9
Control	47	95.7	2.1	0	0	0	0	0	2.1	95.7
Total	95	96.8	1.1	0	0	0	0	0	2.1	96.8
Los Angeles										
Experimental	52	100.0	0	0	0	0	0	0	0	100.0
Control	50	88.0	4.0	0	4.0	2.0	0	0	2.0	89.8
Total	102	94.1	2.0	0	2.0	1.0	0	0	1.0	95.0
New York										
Experimental	58	98.3	0	0	0	0	0	0	1.7	98.3
Control	57	98.3	1.7	0	0	0	0	0	0	98.3
Total	115	98.3	0.9	0	0	0	0	0	0.9	98.3
St. Paul										
Experimental	27	100.0	0	0	0	0	0	0	0	100.0
Control	27	100.0	0	0	0	0	0	0	0	100.0
Total	54	100.0	0	0	0	0	0	0	0	100.0
Tucson										
Experimental	51	100.0	0	0	0	0	0	0	0	100.0
Control	50	98.0	0	2.0	0	0	0	0	0	98.0
Total	101	99.0	0	1.0	0	0	0	0	0	99.0
Total										
Experimental	236	99.1	0	0	0	0	0	0	0.8	99.1
Control	231	95.7	1.7	0.4	0.9	0.4	0	0	0.9	96.1
Total	467	97.4	0.9	0.2	0.4	0.2	0	0	0.9	97.6

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[a] Defined as the number of completed interviews divided by the total number assigned less those sample members who were incarcerated or deceased, or had moved out of the study area.

TABLE B.2

SAMPLE MEMBER 6-MONTH INTERVIEW
FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS
(Percent)

	Total Assigned	Final Status							Response Rate[a]
		Complete/ Partial Complete	Sample Member Refused	Parent/ Guardian Refused	Incar- cerated	Moved Out of Area	Deceased	Unable to Locate or Contact	
Cincinnati									
Experimental	32	90.6	0	0	6.3	3.1	0	0	100.0
Control	30	96.7	0	0	0	0	0	3.3	96.7
Total	62	93.5	0	0	3.2	1.6	0	1.6	98.3
Los Angeles									
Experimental	34	88.2	5.9	0	2.9	2.9	0	0	93.7
Control	30	90.0	3.3	0	3.3	0	0	3.3	93.1
Total	64	89.1	4.7	0	3.1	1.6	0	1.6	93.4
New York									
Experimental	37	97.3	2.7	0	0	0	0	0	97.3
Control	38	97.4	0	2.6	0	0	0	0	97.4
Total	75	97.3	1.3	1.3	0	0	0	0	97.3
St. Paul									
Experimental	18	100.0	0	0	0	0	0	0	100.0
Control	18	100.0	0	0	0	0	0	0	100.0
Total	36	100.0	0	0	0	0	0	0	100.0
Tucson									
Experimental	34	97.1	0	0	0	0	2.9	0	100.0
Control	32	96.9	0	0	0	0	0	3.1	96.9
Total	66	97.0	0	0	0	0	1.5	1.5	98.5
Total									
Experimental	155	94.2	1.9	0	2.1	1.3	0.6	0	98.0
Control	148	95.9	0.7	0.7	0.7	0	0	2.0	96.6
Total	303	95.0	1.3	0.3	1.3	0.7	0.3	1.0	97.3

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[a] Defined as the number of completed interviews divided by the total number assigned less those sample members who were incarcerated or deceased, or had moved out of the study area.

TABLE B.3

SAMPLE MEMBER 15-MONTH INTERVIEW FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS
(Percent)

		Final Status							
	Total Assigned	Complete/ Partial Complete	Sample Member Refused	Parent/ Guardian Refused	Incar- cerated	Moved Out of Area	Deceased	Unable to Locate or Contact	Response Rate[a]
Cincinnati									
Experimental	47	87.2	2.1	0	0	6.4	0	4.3	93.2
Control	45	97.8	2.2	0	0	0	0	0	97.8
Total	92	92.4	2.2	0	0	3.3	0	2.2	95.5
Los Angeles									
Experimental	52	90.4	1.9	0	0	7.7	0	0	97.9
Control	44	88.6	0	0	4.5	2.3	0	4.5	95.3
Total	96	89.6	1.0	0	2.1	5.2	0	2.1	96.6
New York									
Experimental	57	87.7	5.3	3.5	0	0	0	3.5	87.7
Control	56	85.7	1.8	7.1	0	3.6	0	1.8	88.9
Total	113	86.7	3.5	5.3	0	1.8	0	2.7	88.3
St. Paul									
Experimental	27	96.3	0	0	0	0	3.7	0	100.0
Control	27	100.0	0	0	0	0	0	0	100.0
Total	54	98.1	0	0	0	0	1.9	0	100.0
Tucson									
Experimental	51	96.1	0	0	0	2.0	2.0	0	100.0
Control	49	89.8	0	0	2.0	6.1	0	2.0	98.0
Total	100	93.0	0	0	1.0	4.0	1.0	1.0	99.0
Total									
Experimental	234	91.0	2.1	0.9	0	3.4	0.9	1.7	95.1
Control	221	91.4	0.9	1.8	1.4	2.7	0	1.8	95.3
Total	455	91.2	1.5	1.3	0.7	3.1	0.4	1.8	95.2

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[a] Defined as the number of completed interviews divided by the total number assigned less those sample members who were incarcerated or deceased, or had moved out of the study area.

TABLE B.4

SAMPLE MEMBER 22-MONTH INTERVIEW FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS (Percent)

		Final Status							
	Total Assigned	Complete/ Partial Complete	Sample Member Refused	Parent/ Guardian Refused	Incar-cerated	Moved Out of Area	Deceased	Unable to Locate or Contact	Response Rate[a]
Cincinnati									
Experimental	47	85.1	2.1	0	2.1	4.3	0	6.4	90.9
Control	45	86.7	4.4	0	0	6.7	0	2.2	92.9
Total	92	85.9	3.3	0	1.1	5.4	0	4.3	91.9
Los Angeles									
Experimental	52	92.3	1.9	0	0	5.8	0	0	98.0
Control	44	86.4	0	0	2.3	6.8	0	4.5	95.0
Total	96	89.6	1.0	0	1.0	6.3	0	2.1	96.7
New York									
Experimental	57	84.2	5.3	3.5	0	0	0	7.0	84.2
Control	56	82.1	1.8	5.4	1.8	3.6	0	5.4	86.8
Total	113	83.2	3.5	4.4	0.9	1.8	0	6.2	85.5
St. Paul									
Experimental	27	96.3	0	0	0	0	3.7	0	100.0
Control	27	100.0	0	0	0	0	0	0	100.0
Total	54	98.1	0	0	0	0	1.9	0	100.0
Tucson									
Experimental	51	92.2	0	0	0	2.0	2.0	3.9	95.9
Control	49	89.8	0	0	2.0	8.2	0	0	100.0
Total	100	91.0	0	0	1.0	5.0	1.0	2.0	97.8
Total									
Experimental	234	89.3	2.1	0.9	0.4	2.6	0.9	3.9	92.9
Control	221	87.8	1.4	1.4	1.4	5.4	0	2.7	94.2
Total	455	88.6	1.8	1.1	0.9	4.0	0.4	3.3	93.5

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[a] Defined as the number of completed interviews divided by the total number assigned less those sample members who were incarcerated or deceased, or had moved out of the study area.

The response rates for the follow-up interview waves[1] were also high, as shown in Tables B.2 through B.4. For example, the overall 22-month response rate was 93.5 percent, with virtually no difference by research status (92.9 percent for the experimental group and 94.2 percent for the control group). Note that in the follow-up interviews the control group was somewhat more willing to be interviewed than was the experimental group, possibly because members of the control group who were not at all interested in the research or who were difficult to interview had not completed a baseline. Moreover, ancedotal reports from the interviewers suggest that a small number of experimental group members became disillusioned with the program and refused to take part in further research interviews. Site differences in response rates for the follow-up interviews were also generally small, although the New York site had higher rates of refusal and unable-to-locate final statuses in the later waves (15- and 22-month follow-ups) than the other sites. By the 22-month, the New York response rate was 85.5 percent overall, 8 percentage points below the overall rate, but still high in comparison with most longitudinal studies.[2]

The excellent overall response rate by sample members (and, as shown later, by proxy respondents) is probably due to a combination of factors: the explanations of the research given by the STETS intake counselors, the advance letters sent by the interviewers to both sample members and their parents or guardians, and the efforts that interviewers made to explain the study during their contact with respondents. The respect and consideration that the interviewers showed toward sample members in giving them an opportunity to speak for themselves may have also encouraged response. The $5 respondent payments were well received and also seemed to contribute to respondents' willingness to be interviewed.

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[1] Follow-up interviews were assigned only for those sample members who had completed the baseline interview. In addition, the 6-month follow-up included only those sample members who were in the two-thirds subsample determined at randomization.

[2] By comparison, the completion rate for the youth sample in the national Supported Work demonstration 18-month follow-up was 74 percent, with a 6 percentage point difference between experimental and control groups. In the New York Supported Work site, the completion rate was 81 percent for the experimental group, and 67 percent for the control group (see Jackson et al., 1979).

Table B.5 presents the elapsed time in days between assignment and the date of completion, by interview wave and research status. The baseline interview with the sample member was completed just over an average of one month after random assignment. Baseline interviews were assigned weekly or biweekly to interviewers from the central office records of randomization. Once the assignment sheet reached the interviewer, he or she had to visit the local STETS office to pick up a copy of the application/enrollment form, which contained contact information and the signed consent form. The advance letters were then mailed to the primary respondents and their parents, and, after the letters were received, telephone and in-person contact attempts were made to schedule appointments. The necessary delays in executing these steps, and the fact that there was a small backlog of sample members, both in the baseline pilot phase and once the full baseline interviewing began, meant that almost 32 days elapsed between assignment and completion at baseline. On average, members of the experimental group were interviewed about 3 days later than control group members who were assigned on the same day. This may have been due to the difficulty in scheduling an appointment around STETS activities.

Every month during follow-up fielding, interviewers were sent a list of those sample members who were eligible to be interviewed that month. While they were instructed to interview the sample member as near as possible to the time of the month which corresponded to the random assignment date, this was not possible with precision. Therefore, in some cases, the follow-up interview was conducted up to 2 weeks before the date which marked the end of the Xth month after random assignment (where X is 6, 15, or 22 months, depending upon the follow-up wave). These cases are not included as negative values in calculating the mean; rather, the absolute value of the difference in the interview date and the date equivalent to X months after random assignment was calculated. This is the number of days by which the interview date varied from the date marking 6,

TABLE B.5

TIME BETWEEN ASSIGNMENT AND SAMPLE MEMBER INTERVIEW COMPLETION AND LENGTH OF INTERVIEW BY INTERVIEW WAVE AND RESEARCH STATUS

	Mean Time Between Assignment and Completion[a] (Days)	Mean Length of Interview (Minutes)
Baseline Interview		
Experimental	33.4	32.7
Control	30.1	30.8
Total	31.8	31.8
6-Month Interview		
Experimental	14.4	24.5
Control	14.5	25.4
Total	14.4	24.9
15-Month Interview		
Experimental	10.3	24.2
Control	9.8	24.0
Total	10.1	24.1
22-Month Interview		
Experimental	11.9	22.2
Control	13.3	23.0
Total	12.6	22.6
Number in Sample		
Baseline	455	423
6-Month	288	288
15-Month	415	415
22-Month	403	403

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[a] The number of days between the date of random assignment at baseline and the equivalent date in the Xth month after random assignment, where X = 6, 15, or 22 months depending upon the follow-up interview wave. Follow-up interview assignments were made at the beginning of the Xth month after random assignment, and interviewers were instructed to complete the assignments by the end of the month. Because of this, some follow-up interviews were conducted before the equivalent date. For that reason, the absolute value of the difference in assignment or equivalent data and the interview date is presented in this table.

15, or 22 months after random assignment. At the follow-up waves, the number of days between assignment and completion was much shorter than at baseline, about two weeks or less on average. There were only small differences by research status, and no consistent pattern or trend emerged across the follow-up waves.

The baseline interview averaged about 32 minutes, although interviewers often spent additional time, apart from the interview itself, to introduce themselves, establish rapport, and explain the study. The follow-up interviews averaged between 23 and 25 minutes depending upon the wave. The follow-up instrument did not contain the baseline questions on parental background. This factor, plus a greater familiarity with the interview process and the questions on the parts of the respondents and the interviewers, accounts for the shorter follow-up interview administration time.

Table B.6 presents information on the interview setting. Most sample member interviews were conducted in the respondent's home, and generally no one else was present during the interview. When someone else was present, it was usually a parent or guardian, and, in the vast majority of cases, the primary respondent's answers did not appear to be influenced by the presence of others. These patterns apply to all interview waves.

Table B.7 reports several observations from interviewers pertaining to the sample members' orientation toward the interview. Interviewers found that the respondents were generally attentive to the interview, cooperative, and self-confident. These patterns did not change over the course of the study. However, interviewers did believe that the respondents' answers were more reliable in later interviewing waves. At baseline, over 88 percent of respondents were described as very reliable or reliable on most items; by the 22-month follow-up, 94 percent were reported equally reliable. This pattern may have been due to interviewers' increased appreciation of the abilities of the respondents, to respondents' greater familiarity with the questions or to greater knowledge of the issues addressed in the interview, or both.

TABLE B.6

LOCATION OF SAMPLE MEMBER INTERVIEW AND PRESENCE OF OTHERS DURING INTERVIEW BY INTERVIEW WAVE
(Percent)

	Baseline Interview	6-month Interview	15-Month Interview	22-Month Interview
Location of Interview[a]				
Sample member's home	60.7	91.0	89.9	90.1
Home of friend or relative	1.3	1.4	2.4	3.5
Agency office	0.4	1.0	2.4	2.0
Elsewhere[b]	2.4	6.6	5.3	4.5
Unknown	35.2	0	0	0
Others Present During Interview[c]				
No one	80.4	86.8	83.9	85.6
Parent/guardian	13.2	8.7	7.2	7.2
Counselor	0	0.3	0.2	0.3
Roommate/friend/spouse	1.3	1.4	2.7	2.2
Other	7.3	3.5	8.7	7.0
Effect of Presence of Others[d]				
No others present	80.4	86.8	83.9	85.6
Sample member's answers were influenced	3.5	2.1	2.2	2.0
Sample member's answers were not influenced	16.0	11.1	14.0	12.4
Number in Sample	455	288	415	403

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[a] The baseline pilot instrument did not ask interviewers to record this information.

[b] Includes public places such as libraries or restaurants.

[c] More than one type of person could have been coded as present.

[d] Based on interviewer judgment.

TABLE B.7

INTERVIEWER OBSERVATIONS ON SAMPLE MEMBERS
BY INTERVIEW WAVE
(Percent)

Observed Characteristics of Sample Members	Baseline Interview	6-month Interview	15-Month Interview	22-Month Interview
Attentiveness During Interview[a]				
1...Mentally alert, attentive	67.7	73.6	61.2	63.3
2...	22.9	19.8	26.0	27.8
3...	7.3	5.2	9.9	7.4
4...	1.8	1.4	2.2	1.2
5...Inattentive	0.4	0	0.7	0.3
Not answered	0	0	0	0
Cooperativeness During Interview[a]				
1...Cooperative	86.4	82.6	75.2	76.2
2...	9.7	12.5	15.9	18.6
3...	2.6	4.2	6.7	4.5
4...	1.0	0.7	1.9	0.5
5...Uncooperative	0	0	0.2	0.3
Not answered	0.2	0	0	0
Self-Confidence During Interview[a]				
1...Self-confident	34.1	46.2	35.2	41.9
2...	43.1	37.9	39.0	37.5
3...	17.8	11.5	17.3	14.6
4...	4.0	3.5	7.2	4.0
5...Insecure	0.7	1.0	1.2	2.0
Not answered	0.4	0	0	0
Reliability of Responses[b]				
Very reliable	49.7	63.5	69.2	73.2
Reliable on most items	39.1	26.4	22.2	20.8
Reliable on some items	9.0	8.3	7.0	3.2
Very unreliable	2.2	1.7	1.7	2.5
Not answered	0	0	0	0.3
Number in Sample	455	288	415	403

[a] Only the two extreme points of the scale were labelled in the interview.

[b] Data from the primary respondent interviews which were judged "very unreliable" or "reliable on some items" and from those with impaired speech were replaced with proxy respondent interview data if available.

2. Proxy Respondents

During the course of the interview with the sample member, interviewers used a number of predetermined checkpoints in the instrument to determine whether a proxy respondent was required, based on missing or inconsistent data on key interview items. The results of these determinations are shown in Table B.8. During the baseline pilot phase, proxy interviews were attempted for all respondents; therefore, the pilot cases are not included in the baseline column. Pilot study results had suggested that about 30 percent of the primary respondent interviews would contain missing or inconsistent data, and therefore would require a proxy interview. In general, a lower percentage of cases actually required a proxy respondent, from 25 percent during the post-pilot baseline to only 13 percent by the 22-month follow-up wave. Moderate variation occurred by site, particularly at baseline, where the percentages of cases which required a proxy interview ranged from about 15 percent in Los Angeles to 44 percent in Tucson. This variation decreased over time to an 11.1 percentage point different at month 22 between New York and St. Paul. This secular trend is consistent with the pattern in interviewers' subjective judgments about the reliability of sample member data. While, overall, the control group sample was more often identified as requiring proxy respondents, this was not true in all sites. Generally, however, the differences between the experimental and control groups in the percentage of cases which required proxy respondents narrowed after the 6-month interview. By the 22-month wave, there was only a 4 percentage point difference on average.

Tables B.9 through B.12 present the final status and response rate results for proxy respondents in each interview wave. In all waves, virtually all of the assigned proxy respondents completed interviews. This held true in all sites and for both the experimental and the control group.

In general, proxy interviews were completed soon after the sample member interview was completed, as shown in Table B.13. The average elapsed time between the interviews was approximately 2 days at all waves. As reported in Table B.14, most proxy interviews were conducted in person. Since interviewers were required to conduct all proxy interviews

TABLE B.8

WHETHER PROXY RESPONDENT REQUIRED, BY INTERVIEW WAVE, SITE, AND RESEARCH STATUS
(Percent)

	Baseline Interview[a]	6-month Interview	15-Month Interview	22-Month Interview
Cincinnati				
Experimental	17.1	13.8	17.1	15.0
Control	25.0	17.2	15.9	15.4
Total	21.1	15.5	16.5	15.2
Los Angeles				
Experimental	13.5	3.3	10.6	8.3
Control	15.9	18.5	20.5	18.4
Total	14.6	10.5	15.1	12.8
New York				
Experimental	36.8	20.0	12.0	6.3
Control	23.8	29.7	14.6	13.0
Total	30.0	23.3	13.3	9.6
St. Paul				
Experimental	40.7	27.8	30.8	19.2
Control	25.9	27.8	18.5	22.2
Total	33.3	27.8	24.5	20.7
Tucson				
Experimental	27.8	18.2	14.3	12.8
Control	61.1	22.6	11.4	11.4
Total	44.4	20.3	12.9	12.1
Total				
Experimental	23.8	15.1	15.5	11.5
Control	26.7	23.2	15.8	15.5
Total	25.3	19.1	15.7	13.4
Number in Sample	297	288	415	403

NOTE: Whether a proxy respondent was required was determined by the interviewer on the basis of the missing or inconsistent responses in the primary respondent interview. The completion of proxy interviews did not necessarily mean that proxy data replaced primary respondent interview data for analysis. The use of proxy data for analyses was determined by explicit rules discussed in Chapter III.

[a] Does not include baseline pilot data; during pilot study, proxy interviews were attempted for all respondents.

TABLE B.9

PROXY BASELINE INTERVIEW
FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS
(Percent)

	Total Assigned	Final Status: Complete/ Partial Complete	Final Status: Proxy Refused	Final Status: Non-English-Speaking	Final Status: Unable to Locate or Contact	Response Rate[a]
Cincinnati						
Experimental	18	94.4	0	0	5.5	94.4
Control	18	100.0	0	0	0	100.0
Total	36	97.2	0	0	2.8	97.2
Los Angeles						
Experimental	7	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	14	100.0	0	0	0	100.0
New York						
Experimental	45	100.0	0	0	0	100.0
Control	39	97.4	0	2.6	0	97.4
Total	84	98.8	0	1.2	0	98.8
St. Paul						
Experimental	10	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	17	100.0	0	0	0	100.0
Tucson						
Experimental	37	100.0	0	0	0	100.0
Control	42	100.0	0	0	0	100.0
Total	79	100.0	0	0	0	100.0
Total						
Experimental	117	99.1	0	0	0.9	99.1
Control	113	99.1	0	0.9	0	99.1
Total	230	99.1	0	0.4	0.4	99.1

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[a] Defined as the number of completed interviews divided by the total number assigned. There were no proxy respondents ineligible for the survey because of death, incarceration, or relocation out of the study area.

TABLE B.10

PROXY 6-MONTH INTERVIEW FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS (Percent)

		Final Status				
	Total Assigned	Complete/ Partial Complete	Proxy Refused	Non-English-Speaking	Unable to Locate or Contact	Response Rate[a]
Cincinnati						
Experimental	4	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	9	100.0	0	0	0	100.0
Los Angeles						
Experimental	1	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	6	100.0	0	0	0	100.0
New York						
Experimental	6	100.0	0	0	0	100.0
Control	11	90.9	0	9.1	0	90.9
Total	17	94.1	0	5.9	0	94.1
St. Paul						
Experimental	5	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	10	100.0	0	0	0	100.0
Tucson						
Experimental	6	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	13	100.0	0	0	0	100.0
Total						
Experimental	22	100.0	0	0	0	100.0
Control	33	97.0	0	3.0	0	97.0
Total	55	98.2	0	1.8	0	98.2

[a] Defined as the number of completed interviews divided by the total number assigned. There were no proxy respondents ineligible for the survey because of death, incarceration, or relocation out of the study area.

TABLE B.11

PROXY 15-MONTH INTERVIEW FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS

(Percent)

		Final Status				
	Total Assigned	Complete/ Partial Complete	Proxy Refused	Non-English-Speaking	Unable to Locate or Contact	Response Rate[a]
Cincinnati						
Experimental	7	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	14	100.0	0	0	0	100.0
Los Angeles						
Experimental	5	100.0	0	0	0	100.0
Control	8	100.0	0	0	0	100.0
Total	13	100.0	0	0	0	100.0
New York						
Experimental	6	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	13	100.0	0	0	0	100.0
St. Paul						
Experimental	8	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	13	100.0	0	0	0	100.0
Tucson						
Experimental	7	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	12	100.0	0	0	0	100.0
Total						
Experimental	33	100.0	0	0	0	100.0
Control	32	100.0	0	0	0	100.0
Total	65	100.0	0	0	0	100.0

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[a] Defined as the number of completed interviews divided by the total number assigned. There were no proxy respondents ineligible for the survey because of death, incarceration, or relocation out of the study area.

TABLE B.12

PROXY 22-MONTH INTERVIEW FINAL STATUS AND RESPONSE RATE BY SITE AND RESEARCH STATUS
(Percent)

	Total Assigned	Final Status: Complete/ Partial Complete	Final Status: Proxy Refused	Final Status: Non-English-Speaking	Final Status: Unable to Locate or Contact	Response Rate[a]
Cincinnati						
Experimental	6	100.0	0	0	0	100.0
Control	6	100.0	0	0	0	100.0
Total	12	100.0	0	0	0	100.0
Los Angeles						
Experimental	4	100.0	0	0	0	100.0
Control	7	100.0	0	0	0	100.0
Total	11	100.0	0	0	0	100.0
New York						
Experimental	3	100.0	0	0	0	100.0
Control	6	100.0	0	0	0	100.0
Total	9	100.0	0	0	0	100.0
St. Paul						
Experimental	6	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	11	100.0	0	0	0	100.0
Tucson						
Experimental	6	100.0	0	0	0	100.0
Control	5	100.0	0	0	0	100.0
Total	11	100.0	0	0	0	100.0
Total						
Experimental	25	100.0	0	0	0	100.0
Control	29	100.0	0	0	0	100.0
Total	54	100.0	0	0	0	100.0

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[a] Defined as the number of completed interviews divided by the total number assigned. There were no proxy respondents ineligible for the survey because of death, incarceration, or relocation out of the study area.

TABLE B.13

AVERAGE TIME BETWEEN SAMPLE MEMBER AND PROXY INTERVIEWS, BY INTERVIEW WAVE AND RESEARCH STATUS

(Days)

	Experimental Group	Control Group	Total
Baseline Interview	1.6	1.8	1.7
6-Month Interview	0.9	3.6	2.5
15-Month Interview	2.4	1.8	2.1
22-Month Interview	1.9	2.3	2.2
Number in Sample			
Baseline	117	114	231
Month 6	22	33	55
Month 15	33	32	65
Month 22	24	30	54

NOTE: Average is defined as arithmetic mean. Only those cases in which the interviewer determined that a proxy respondent interview was required and it was completed are included in this table. Whether a proxy respondent was required was determined by the interviewer on the basis of missing or inconsistent responses in the primary respondent interview. The completion of proxy interviews did not necessarily mean that proxy data replaced primary respondent interview data for analysis. Cases in which the proxy respondent interview was administered immediately after, or on the same day as, the primary respondent interview are coded as "zero" elapsed days.

TABLE B.14

MODE OF PROXY INTERVIEW ADMINISTRATION AND TYPE OF RESPONDENT BY INTERVIEW WAVE
(Percent)

	Baseline Interview	6-Month Interview	15-Month Interview	22-Month Interview
Mode of Administration				
In person	93.4	87.0	87.7	81.5
By telephone	6.6	13.0	12.3	18.5
Type of Respondent				
Parent	74.2	72.7	63.1	61.1
Foster parent	4.4	9.1	6.2	5.5
Legal guardian	0	0	1.5	1.9
Other relative	6.1	5.5	0	3.7
Roommate or friend	0.9	0	0	0
Residential house parent	4.8	1.8	7.7	9.3
Agency staff member	9.2	10.9	21.5	13.0
Other	0.4	0	0	5.5
Number in Sample	229	55	65	54

in person during the baseline pilot study, the table includes only those baseline interviews completed after the pilot study. Even so, proxy respondents were overwhelming interviewed in person throughout the study, due largely to the fact that very often the proxy interview immediately followed the in-person interview with the sample member.

Table B.14 also reports the distribution of proxy respondents by relationship to the primary respondent. In any given interview wave, most proxy respondents were parents of the sample members. However, in later (15- and 22-month) waves, over 20 percent of the proxy respondents were agency staff or residential counselors.

3. Missing Data

The amount of missing data (don't know responses or unusable responses) is a critical measure of the quality of response in self-reported interviews. Previous research with mentally retarded populations and our own experience in the baseline pilot study led us to expect missing data from sample members on certain items that were central to the evaluation. These items were generally those that pertained to the amount of money received from earnings and benefit programs, as well as to the identity of the specific programs from which cash and other benefits were received. This expectation was confirmed by the percentage of missing data on key items, as reported in Tables B.15 through B.18. The first column of percentages in these tables is the percentage of responses recorded as "don't know" or uncodable. (Refusals to provide information are not treated as missing data in these tables.) For the sample members themselves, the items on which 5 percent or more of the data were missing were those pertaining to transfer program benefit receipt and the amount of earnings. There was very little missing data on activities or details of living arrangements. These patterns held true in all waves, with the amount of SSI or SSDI benefits consistently having the highest level of missing data. This is not surprising, given the fact that, in many cases, these benefits were handled by someone else on behalf of the sample member.

What is more surprising, perhaps, is the fact that proxy respondents were not necessarily more knowledgeable. Particularly in terms

TABLE B.15

BASELINE INTERVIEW
MISSING DATA BY SOURCE
(Percent)

Category of Variable	Variable	Sample Member Interviews	Proxy Interviews	Combined Data[a]
Labor Market Outcomes	Employment Status[b]	0.2	1.7	0.4
	Hours Worked per Week[c]	2.0	10.9	1.5
	Weekly Earnings[c]	5.5	28.4	4.6
Training and Schooling	In Training[b]	3.7	6.5	2.9
	In School[b]	0	0	0.2
Public Transfer Dependence	Receipt of SSI or SSDI[d]	11.2	3.5	1.1
	Receipt of Other Cash Transfers[d]	5.7	1.7	0.7
	Receipt of Food Stamps[d]	4.2	2.6	0.7
	Receipt of Medicare or Medicaid[d]	5.9	3.5	0.9
	Amount of SSI or SSDI[e]	15.8	5.7	1.3
	Amount of Other Cash Transfers[e]	8.8	1.7	2.4
Independence and Life Style	Financial Management Activities			
	Pays for purchases by self	0.2	0.4	0.4
	Banks by self	0.7	0.4	0.4
	Pays bills by self	1.3	2.2	1.8
	Living Arrangements	0.7	0.4	0.2
Number in Sample		455	229	455

NOTE: Missing data refers only to "don't know" responses or other responses indicating an inability to provide accurate information (e.g., vague, uncodable, or unintelligible responses). Refusals to provide the requested information are not included as missing data.

[a] Data are combined after identifying the items on which the sample member data were missing and after replacing them with proxy data if available.

[b] A "don't know" response by the sample member was an indicator to use proxy data for the entire work/school/training module.

[c] A "don't know" response by the sample member was an indicator to use proxy data for this variable, except for certain special circumstances.

[d] Based on the pilot study results, "don't know" responses by sample members to questions about the receipt of transfers were treated as no receipt for the purposes of analysis. However, "don't know" responses on these items are included as missing in this table.

[e] A "don't know" response to any question on the amount of transfer received was an indicator to use proxy data for all information on the receipt of transfers.

TABLE B.16

6-MONTH FOLLOW-UP INTERVIEW MISSING DATA BY SOURCE (Percent)

Category of Variable	Variable	Sample Member Interviews	Proxy Interviews	Combined Data[a]
Labor Market Outcomes	Employment Status[b]	0	1.8	0
	Hours Worked per Week[c]	1.4	12.7	0.7
	Weekly Earnings[c]	4.5	16.4	1.7
Training and Schooling	In Training[b]	0.3	3.6	0.3
	In School[b]	0	0	0
Public Transfer Dependence	Receipt of SSI or SSDI[d]	9.4	3.6	0.3
	Receipt of Other Cash Transfers[d]	6.9	0	0
	Receipt of Food Stamps[d]	4.9	0	0
	Receipt of Medicare or Medicaid[d]	2.4	0	0
	Amount of SSI or SSDI[e]	10.8	3.6	0.3
	Amount of Other Cash Transfers[e]	4.2	1.8	1.4
Independence and Life Style	Financial Management Activities			
	Pays for purchases by self	0	1.8	0
	Banks by self	1.0	0	1.0
	Pays bills by self	0.7	0	0.7
	Living Arrangements	0.3	0	0
Number in Sample		288	55	288

NOTE: Missing data refers only to "don't know" responses or other responses indicating an inability to provide accurate information (e.g., vague, uncodable, or unintelligible responses). Refusals to provide the requested information are not included as missing data.

[a] Data were combined after identifying the items on which the sample member data were missing and after replacing them with proxy data if available.

[b] A "don't know" response by the sample member was an indicator to use proxy data for the entire work/school/training module.

[c] A "don't know" response by the sample member was an indicator to use proxy data for this variable, except for certain special circumstances.

[d] Based on the pilot study results, "don't know" responses by sample members to questions about the receipt of transfers were treated as no receipt for the purposes of analysis. However, "don't know" responses on these items are included as missing in this table.

[e] A "don't know" response to any question on the amount of transfer received was an indicator to use proxy data for all information on the receipt of transfers.

TABLE B.17

15-MONTH FOLLOW-UP INTERVIEW
MISSING DATA BY SOURCE
(Percent)

Category of Variable	Variable	Sample Member Interviews	Proxy Interviews	Combined Data[a]
Labor Market Outcomes	Employment Status[b]	0.2	1.5	0
	Hours Worked per Week[c]	2.2	6.1	0.5
	Weekly Earnings[c]	6.5	21.5	3.1
Training and Schooling	In Training[b]	0.2	3.1	0.2
	In School[b]	0	0	0
Public Transfer Dependence	Receipt of SSI or SSDI[d]	4.3	1.5	0
	Receipt of Other Cash Transfers[d]	4.6	1.5	0
	Receipt of Food Stamps[d]	2.7	0	0
	Receipt of Medicare or Medicaid[d]	3.4	3.1	0.2
	Amount of SSI or SSDI[e]	9.4	10.8	1.7
	Amount of Other Cash Transfers[e]	3.6	1.5	0.5
Independence and Life Style	Financial Management Activities			
	Pays for purchases by self	0	0	0
	Banks by self	1.0	1.5	0.7
	Pays bills by self	0.7	1.5	0.7
	Living Arrangements	1.0	0	0.2
Number in Sample		415	65	415

NOTE: Missing data refers only to "don't know" responses or other responses indicating an inability to provide accurate information (e.g., vague, uncodable, or unintelligible responses). Refusals to provide the requested information are not included as missing data.

[a] Data were combined after identifying the items on which the sample member data were missing and after replacing them with proxy data if available.

[b] A "don't know" response by the sample member was an indicator to use proxy data for the entire work/school/training module.

[c] A "don't know" response by the sample member was an indicator to use proxy data for this variable, except for certain special circumstances.

[d] Based on the pilot study results, "don't know" responses by sample members to questions about the receipt of transfers were treated as no receipt for the purposes of analysis. However, "don't know" responses on these items are included as missing in this table.

[e] A "don't know" response to any question on the amount of transfer received was an indicator to use proxy data for all information on the receipt of transfers.

TABLE B.18

22-MONTH FOLLOW-UP INTERVIEW
MISSING DATA BY SOURCE
(Percent)

Category of Variable	Variable	Sample Member Interviews	Proxy Interviews	Combined Data[a]
Labor Market Outcomes	Employment Status[b]	0.2	0	0
	Hours Worked per Week[c]	2.0	5.6	0.3
	Weekly Earnings[c]	4.5	5.6	0.7
Training and Schooling	In Training[b]	0.7	1.9	0.5
	In School[b]	0	0	0
Public Transfer Dependence	Receipt of SSI or SSDI[d]	4.0	7.4	1.0
	Receipt of Other Cash Transfers[d]	3.0	1.9	0.3
	Receipt of Food Stamps[d]	1.7	0	0
	Receipt of Medicare or Medicaid[d]	4.0	1.9	0.3
	Amount of SSI or SSDI[e]	8.9	5.6	1.0
	Amount of Other Cash Transfers[e]	2.2	0	0.5
Independence and Life Style	Financial Management Activities			
	Pays for purchases by self	0.2	1.9	0
	Banks by self	0.5	0	0.3
	Pays bills by self	1.0	1.9	0.7
	Living Arrangements	0.5	0	0
Number in Sample		403	54	403

NOTE: Missing data refers only to "don't know" responses or other responses indicating an inability to provide accurate information (e.g., vague, uncodable, or unintelligible responses). Refusals to provide the requested information are not included as missing data.

[a] Data are combined after identifying the items on the which sample member data were missing and after replacing them with proxy data if available.

[b] A "don't know" response by the sample member was an indicator to use proxy data for the entire work/school/training module.

[c] A "don't know" response by the sample member was an indicator to use proxy data for this variable, except for certain special circumstances.

[d] Based on the pilot study results, "don't know" responses by sample members to questions about the receipt of transfers were treated as no receipt for the purposes of analysis. However, "don't know" responses on these items are included as missing in this table.

[e] A "don't know" response to any question on the amount of transfer received was an indicator to use proxy data for all information on the receipt of transfers.

of hours and earnings from jobs, proxy respondents consistently reported more missing data than did the sample members who had been identified as being unable to provide complete and consistent information on their own.

In fact, not all of the proxy data available proved necessary in constructing the analysis file. In some cases, the proxy data were no better than the sample member data for missing or inconsistent items; in other cases, sample member data were accurate and complete for most items and needed supplementation from the proxy interview only for a few items; in still others, the entire case had to be based on proxy data. As summarized in Table III.7 in Chapter III, only about 6 percent of the sample member interviews were replaced entirely with data from proxy respondents. Substantial proportions (up to 16 percent) used proxy data for the transfer benefit module,[1] although this percentage declined (to 7 percent) over the interviewing period. Once the two sources--sample member and proxy respondent data--were merged, very little missing data remained, as is shown in the last column of Tables B.15 through B.18.

D. SUMMARY: METHODOLOGICAL CONTRIBUTIONS

The rigorous evaluation of the STETS demonstration makes an important contribution to research and policy-making in the area of programs for the mentally retarded. The data collection effort associated with the evaluation also marks a milestone in research with this population. The STETS data collection is probably the largest systematic effort to obtain the data necessary for quantitative analysis directly from mentally retarded persons themselves. Certainly, there were limitations on the data which seemed feasible to collect in a self-reported interview--in particular, data on past experiences--and it was necessary in some cases to collect and use data from parents, counselors, and other proxy respondents. However, the great majority of mentally retarded persons in our sample provided complete and accurate[1] information on all variables of interest. While the same level of success cannot be guaranteed with all mentally retarded persons, this study should make self-reported interviews a serious option in future research efforts, and provide a model for questionnaire design and data collection procedures to maximize the quality of self-reported data.

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[1] Based on the pilot study results, "don't know" responses by sample members to questions about the receipt of transfers were treated as no receipt for the purposes of analysis.

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[1] Accuracy was evaluated in the pilot study through comparisons of sample member responses with those of proxy respondents and information in STETS intake records; it was evaluated during the full study through comparisons of self-reports of activities and types of services received with program records and service agency interviews.

APPENDIX C

BENEFIT-COST ANALYSIS METHODS AND RESULTS

This appendix describes the estimates and valuation assumptions that underlie the benefit-cost findings presented in Chapter VIII. Those findings reflect both numerous assumptions about the effects of STETS on participants' behavior and the appropriate values of those effects. While we feel that these assumptions are reasonable, an inherent uncertainty in the analysis remains. The information presented in this appendix on the rationale and use of these underlying assumptions and on the sensitivity of the results to changes in these assumptions provides a basis for judging this inherent uncertainty and the validity of the overall findings.

Throughout the analysis, we emphasized that the benefit-cost analysis represents a method for drawing together information on the diverse impacts and costs of STETS. It compares the estimated values of benefits and costs in order to help form judgments about the effectiveness of the program overall. Thus, the individual dollar estimates of benefits and costs are intended to suggest the order-of-magnitude of these values, rather than to represent precise measures of value, implying that attention should be paid to the pattern of estimates obtained in analysis, and not to the specific dollar estimates.

Our review of the underlying assumptions is organized into the following six sections:

A. General valuation procedures

B. Program cost estimates

C. The value of Phase 1 and Phase 2 output

D. Impact estimates for the benefit-cost analysis

E. Estimated values for impacts

F. Sensitivity tests

The last section examines how the overall findings would be affected by changes in the valuation assumptions and procedures. It therefore provides a method for summarizing and assessing the level of uncertainty inherent in the analysis.

A. GENERAL VALUATION PROCEDURES

Chapter VIII presented our basic approach to the benefit-cost analysis. The approach is similar to the approaches used in evaluations of the national Supported Work demonstration (Kemper, Long, and Thornton, 1981 and 1984) and Job Corps (Thornton, Long, and Mallar, 1982). It also uses basic benefit-cost accounting procedures, which are described in Gramlich (1981) and other texts on benefit-cost analysis.

The analysis attempts to estimate the effect of STETS on the use of resources and the distribution of resources between the participant group and the nonparticipant group (i.e., all other persons in society). It does so by estimating the market value of any use, creation, or savings of resources. For example, increased output produced by participants when they leave STETS is valued on the basis of what employers pay those participants to produce the output. Similarly, when participants reduce their use of programs other than STETS, we estimate the value of the resulting resource savings on the basis of what it would have cost the affected program to provide the foregone services. This focus on resource use is similar to the approach used to calculate gross national product.

Of course, the focus on resource costs fails to capture a number of potentially important intangible benefits that are listed in the benefit-cost accounting framework (Table VIII.1). For example, the preferences of participants to be employed productively or to be more self-sufficient are not captured by the resource-cost approach. Similarly, the altruistic attitudes of nonparticipants are excluded. However, because these and the other intangible benefits and costs represent important objectives and impacts of STETS, we have made an effort to include them in the analysis, even though they are not explicitly valued. Therefore, throughout the analysis, it is important to remember that the resource-cost approach captures only some of the components of the accounting framework; judgments about program effectiveness must reflect all the components and thus must consider the dollar value of changes in resource use and the potential magnitude of intangible benefits and costs.

In using the resource-cost approach, we corrected for the effects of inflation on the dollar value of goods and services. The data used in the evaluation cover the period from late 1981 to fall 1984. During this period, the general price level, as measured by the implicit price deflator for gross national product, rose by 21 percent.[1] Thus, the actual goods and services represented by a dollar changed over the period. To ensure that our benefit-cost estimates reflect changes in resource use rather than in price levels, we value all benefits and costs in 1982 dollars.

This base period was chosen because it corresponds to the period of "steady-state" operations identified by Riccio and Price (1984; 127ff), and because the program cost estimates are based on data from this period. By using 1982 as the base period, we did not have to adjust the program cost data. For other dollar-denominated benefits and costs (for example, earnings and transfer payments), we multiplied the amounts reported in the interviews by the percentage change in the implicit price deflator between 1982 and the interview date. The other benefits and costs, such as months in a sheltered workshop or school, were valued by multiplying the change in months of use by a "shadow price" that indicated the monthly cost of using that program in 1982 dollars. Readers who are interested in the current value of the benefits and costs discussed in this report can estimate that value by multiplying our estimates by the change in the implicit price deflator between 1982 and the current period. For example, to translate the 1982 dollar estimates into first-quarter 1985 dollars, one would have to increase the figures presented in the benefit-cost analysis by 10 percent, which is the approximate percentage change in the implicit price deflator between 1982 and the first quarter of 1985.

An additional adjustment beyond the inflation adjustments is necessary before benefits and costs that occur at different times can be compared. This adjustment discounts values over time to their equivalent present value. Discounting is necessary because a benefit or cost

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[1] The implicit price deflator for gross national product is used, rather than other indicators of price levels, because it is more broadly based. The implicit price deflator essentially reflects prices for all commodities used in the economy rather than prices for a specific set of goods, as is the case with price indexes such as the consumer price index.

(measured as a given amount of dollars) achieved this year is worth more than one achieved, say, ten years from now, even after inflation has been taken into account. Consider a result that increased participant earnings. These increased earnings, if they occurred this year, could be reinvested and could earn a rate of return over the next ten years. Thus, over a ten-year period, the value of the increase in earnings will equal the initial increase plus the return on investment over the next ten years. This value will clearly exceed the value of the same increase if it occurred ten years from now.[1]

Because of these differences in value, the analysis adjusts all estimated benefits and costs to equivalent values by discounting those that occur in the future by a factor that reflects the return that could have been earned in the interim. The resulting discounted values are termed "present values." We use the time of randomization as our base period for discounting. Thus, all discounted values presented in the report indicate the present value at the time of random assignment (i.e., enrollment in the demonstration).

The appropriate discount rate in evaluating social programs is always somewhat controversial because, although the choice of a discount rate is very important for the evaluation and is well established theoretically, there has never been a completely satisfactory way to estimate discount rates.[2] Imperfections in the markets for capital, the existence of risk, uncertainty, and inflation, and the fact that many tax-incidence questions are still unresolved have made it impossible to determine a single discount rate that is appropriate for evaluating social programs. Consequently, the discount rate is typically chosen arbitrarily. Most studies of social programs have used rates of between 3 and 10 percent a year. Our procedure is to assume a middle value, 5 percent, and then to test the sensitivity of the findings to this assumption by recomputing the values using 3 and 10 percent discount rates.[1]

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[1] Suppose that a \$1,000 benefit occurs 10 years from now. What present value invested at 5 percent return per annum would yield \$1,000 ten years from now? Call that value PV. PV invested today would earn 5 percent a year for 10 years, or $(1 + .05)^{10}$. Thus, its value 10 years from now is PV $(1 + .05)^{10}$ equal to \$1,000. Divide both sides by $(1 + .05)^{10}$ to obtain the present value = $\frac{\$1,000}{(1 + .05)^{10}}$, or \$614. This figure is the present value of a \$1,000 benefit that would occur 10 years from now.

[2] Baumol (1968) provides a theoretical foundation for measuring the social discount rate. He suggests that it should measure the rate of return that the resources used for the public investment would have earned otherwise in the private sector. Bradford (1975) suggests using the rate at which consumers trade off future for current consumption (the social rate-of-time preference). These approaches lead to the same rate if all markets are competitive. However, in the presence of markets that are characterized by monopoly power, inflation, taxes, and uncertainty, the approaches lead to quite different results and are difficult to implement empirically.

For employment and training programs such as STETS, the social net present value (the basic benefit-cost criterion) will change in an opposite direction from a change in the discount rate, because social costs are generally incurred during the in-program period (hence, their value is not changed much by discounting to the time of enrollment), while the benefits accrue over many time periods. Therefore, if a higher rate is used, the present value of future benefits will fall, and, because costs are essentially unaffected, estimated net present value will decline.

B. PROGRAM COST ESTIMATES

As stated in Chapter VIII, program costs consist of three elements: operational costs, participant compensation, and central administrative costs. In analyzing all three types of costs, we recognize that the cost estimates must be consistent with the impact estimates. In particular, the costs should reflect the resources required to generate the impacts. We want to include the costs of any program activity that has affected participant behavior and to exclude the costs of activities that did not affect the impacts.

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[1] The 10 percent rate is mandated by the Office of Management and Budget (1972) for evaluating government investments. All the discount rates are net of inflation; given current inflation rates of 4 percent per year, the 3 to 10 percent range in real rates corresponds to nominal interest rates of 7 percent to 14 percent.

What costs, if any, should be excluded depends on (1) the costs of the research and evaluation component of STETS and (2) the extraordinary costs of program operation due to the fact that STETS was a demonstration. The activities associated with these costs are not part of the basic STETS model and would not be incurred in an ongoing program. Moreover, it was assumed that these activities did not affect the delivery of services to participants or the subsequent behavior of participants. Consequently, we excluded these costs from the cost estimates presented in Chapter VIII.

This assumption seems quite reasonable in terms of the research costs. Most of these costs at the project level pertained to the additional outreach and screening efforts necessary to identify individuals who were ultimately assigned to the control group, and to the efforts necessary to complete the evaluation components of the client and cost-monitoring forms. Neither of these activities was likely to have affected the observed impacts.

The assumption is more tenuous in terms of the extraordinary costs incurred by the projects because STETS was operated as a demonstration. Many of these costs were attributable to start-up activities and to the small scale of the demonstration, and it is plausible that a larger ongoing program would be able to provide STETS services at a lower average cost. The uncertainty of the assumption exists particularly because the small scale may have enabled the demonstration programs to provide more intensive services. This scenario could have occurred, for example, during the start-up and phase-down periods of the demonstration, when the ratio of staff to clients was higher than during the steady-state period. If more intensive services were provided, then the extra costs associated with those services should be included.

We have no way of knowing whether more intensive services were delivered or what they cost. Even if the services could be identified, it would be extremely difficult to separate the costs of any intensive services from the normal start-up costs and from the costs due to the relatively small scale of operations. Nevertheless, by using an upper-bound estimate of costs that includes all operating costs of the demonstration rather than only those costs that were incurred during the steady-state period, we can test the sensitivity of our overall findings to changing the assumption that the extraordinary demonstration operating costs did not affect program impacts.

We estimated both steady-state and total costs by using data provided by MDRC's demonstration accounting system. To estimate steady-state average cost per participant, we first subtracted estimated research costs from steady-state operating costs. We then divided by the number of active months for the period and multiplied the result by the average active months per participant for the entire demonstration period.[1] The resulting estimates, which are presented in Table C.1, indicate what it would have cost to serve an average STETS participant had all participants been served during the steady-state period. Using a similar procedure to estimate the costs for the entire demonstration period, we find that the average operating costs per participant for the entire demonstration (excluding estimated research costs) exceeded the steady-state operating costs by 32 percent ($8,221, compared with $6,211). The data also indicate that participant compensation per participant was lower over the entire demonstration than during the steady-state period. Section F examines how these differences affect the overall benefit-cost conclusions.

In contrast to operational costs and participant compensation, the central administrative costs were not estimated on the basis of the demonstration experience. As we noted in Chapter VIII, MDRC's dual role as monitor and researcher made it impossible to identify the costs of central

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[1] We also could have used enrollment months in making this calculation. Doing so would not have altered the estimate of cost per participant. While average costs per enrollment month are lower than average costs per active month, the average length of participation is correspondingly greater when measured in enrollment months rather than in active months. We present the costs per enrollment month in Table C.1 to facilitate comparisons with the net cost per service year estimates in Table 7.1 of Riccio and Price (1984); their net cost per service year estimate ($8,715), when converted to a monthly amount by dividing by twelve, equals operating costs per enrollment month ($552.5) plus participant compensation per enrollment month ($282.6) less service project revenue per enrollment month ($108.8).

TABLE C.1

PROGRAM COST ESTIMATES FOR THE BENEFIT-COST ANALYSIS

	Steady-State Period			Entire Demonstration Period		
	Operating Cost	Participant Compensation	Service Project Revenue[a]	Operating Cost	Participant Compensation	Service Project Revenue[a]
Total for Period	593,498	281,154	108,267	2,496,652	740,079	318,518
Research Costs[b]	43,733	--	--	161,837	--	--
Net for Period	549,765	281,154	108,267	2,334,815	740,079	318,518
Average Per Enrollment Month[c]	552.5	282.6	108.8	783.0	248.2	106.8
Average Per Active Month[d]	666.4	341.0	131.2	976.6	310.0	133.2
Net Per Participant[e]	6,211	3,176	1,223	8,221	2,606	1,122

[a] Service project revenue includes payments received by the projects in exchange for goods produced by participants or as compensation for participant labor. For the steady-state period, the service project revenue was assumed to be obtained with a three-month lag (see Riccio and Price, 1984, p. 128, footnote 1).

[b] Research costs were estimated to be 5 percent of total project-level costs (operating costs plus participant compensation); see Riccio and Price (1984) p. 128, footnote 3.

[c] For the steady-state period, there were 995 enrollment months. For the demonstration as a whole, there were 2,982 enrollment months.

[d] For the steady-state period, there were 825 active months. For the demonstration as a whole, there were 2,391 active months.

[e] Cost per participant for the steady-state period is derived by multiplying the average costs per active month by 9.32, the average length of participation in active months for research sample members. For the demonstration as a whole, we simply divided total costs by the number of participants, 284. These costs are not discounted.

administration. We thus estimated these costs on the basis of central administrative expenses for existing employment and training programs funded by the U.S. Department of Labor (DOL). In fiscal 1982, central administrative costs for the Employment and Training Administration at DOL were approximately 2 percent of total program expenditures (see the Budget of the United States Government, Fiscal 1984, pp. I-01 to I-08). If this ratio is applied to the steady-state project-level costs (operational costs plus participant compensation), the estimated central administrative costs would be approximately $20 per active month. This number represents the general order-of-magnitude of central administrative costs; the actual costs that would be incurred if STETS were implemented on a permanent basis may differ according to the level of central monitoring and technical assistance provided.

C. THE VALUE OF PHASE 1 and PHASE 2 OUTPUT

We estimated the value of output produced by participants on Phase 1 and Phase 2 jobs by studying the work performed by a randomly selected subset of participants. These studies were conducted between September and December 1982, a period that corresponds approximately with the steady-period.[1] We selected eight active participants from each site and examined the output they produced over a two-week period.[2] As part of this examination, we interviewed program staff and the participants' work supervisors and consulted actual production records. We collected detailed production information that included hours worked, wages paid, supervision provided, and the amount of work completed.

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[1] The steady-state period differed slightly across sites (Riccio and Price, 1984, p. 127), but generally ran from June to October 1982. Our value-of-output measurement periods also differed across sites, from late September 1982 in Cincinnati to mid-December in Los Angeles.

[2] We studied a total of 40 participants. Of this total, 33 were in the research sample, and the others had been enrolled prior to the start of randomization. The results for the full sample of 40 and for the subsample of 33 are essentially the same. Both sets of results are presented, but we focus on the results for the research sample members.

We made two estimates of the value of output produced. The first estimate was the alternative supplier's price of the participant output--that is, what an employer would have paid other workers to produce the participant output had the participant been unavailable. This estimate assumes that employers could obtain additional labor at the wages paid to their regular employees, and it ignores any additional costs that may have been imposed on the employers who hired STETS participants. The second estimate attempts to deduct these additional costs. It estimates the net value added by subtracting from the alternative supplier's price the costs of extra employer-provided supervision and any reduced output from other workers.

The net value may differ from the alternative supplier's price for several reasons. Participants may have required supervision or training beyond that which would usually be provided to regular workers. For example, one firm which used a participant to clean its offices received very little net value, even though the participant performed good work at a productive pace. That firm provided considerable training and was unwilling to scale back its purchase of alternative cleaning services until it could dispense with these services altogether. Alternatively, an employer may choose to use the participant labor (often provided at subsidized wages) in a way that fails to maximize output. An example of this scenario was a firm in which the regular clerical staff "donated" their simplest jobs to the participant--photocopying, mailing, and answering the telephone during employee breaks. The work performed by this participant was both useful and necessary, but reduced the efforts of other employees. Finally, the work performed by participants may affect the quality of output produced, but the employer may be unable to capture the value of this increased quality by increasing prices or output. This was the case for a participant who worked at a pre-school and appeared to provide extra services and more individual attention to the students. However, the school did not serve any additional students because of the participant, and it did not receive any extra revenue. The alternative supplier's price was just over the minimum wage, but the net value was zero because no increase occurred in the aggregate value of output. Thus, in some cases, the net value fails to capture quality changes.

The net supply price is the appropriate concept when using the resource cost approach. Nevertheless, the gross supply price is useful for assessing the potential of participants to be productive. Table C.2 presents both estimates.

The table also presents the average participant compensation per active month and the average service project revenue per active month. These figures, which correspond to the steady-state period, provide an interesting basis for interpreting the value-of-output estimates, particularly when all value-of-output estimates are made on a per-participant basis. As shown in Table C.2, average revenue was only 35 percent of the average net value added. However, average participant compensation was only slightly less than our estimate of net value added.

In assessing how assumptions about the value of Phase 1 and Phase 2 output affect the overall beneft-cost results, we used the average revenue estimate as a lower bound. We used the net value added for the research sample as our preferred estimate. The average alternative supplier's price for the research sample provides a likely upper bound. Section F examines how these three alternative values affect the overall results.

D. IMPACT ESTIMATES FOR THE BENEFIT-COST ANALYSIS

The impact estimates used in the benefit-cost analysis are essentially those that were presented in Chapters IV through VII, with three differences. First, as we stated in Chapter VIII, we do not use the three separate point-in-time estimates of impacts at 6, 15, and 22 months after randomization. Instead, we linearly interpolate between those point-in-time estimates in order to estimate the cumulative impact over the entire 22-month observation period. Second, for dollar-denominated impacts (earnings, income, SSI/SSDI payments, and other welfare payments), we measured impacts in 1982 dollars rather than in current dollars, which were used in the impact analysis (this procedure was described in Section A).

TABLE C.2

ALTERNATIVE ESTIMATES OF THE VALUE OF OUTPUT PRODUCED ON PHASE 1 AND PHASE 2 JOBS

Valuation Method	Value Per Active Month			Average Value Per Participant[a]
	Phase 1	Phase 2	Both Phases	
Net Value Added[b]				
Full sample	281	498	n.a.	3,446
Research sample	293	503	n.a.	3,531
Alternative Supplier's Price[b]				
Full sample	326	681	n.a.	4,391
Research sample	346	707	n.a.	4,578
Steady-State Phase Revenue[c]	n.a.	n.a.	131	1,223
Steady-State Participant Compensation	n.a.	n.a.	341	3,176

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[a] The per-participant estimates are made by multiplying the estimates of the value of output per active month by the average length of participation. On average, participants spent 5.51 months active in Phase 1 and 3.81 months active in Phase 2, for a total length of stay of 9.32 months. These estimates are not discounted.

[b] "Alternative supplier's price" is the alternative labor cost of producing the output produced by the participant. "Net value added" subtracts out any additional supervision or lost productivity costs incurred by employers who provide Phase 1 and Phase 2 jobs. These values-of-output estimates are based on a randomly selected sample of 40 participants; 33 of these selected participants were in the STETS research sample.

[c] Project revenue was received with a lag. The figure in the table assumes that the lag was three months after the work was performed (Riccio and Price, 1984). If no lag was assumed, the project revenue per active month would have been $106 during the steady-state period.

n.a. = not available.

Third, we disaggregated the living arrangement, schooling, and training variables differently than was done in the impact analysis, so as to enable us to examine changes in the use of specific types of these programs and to value those changes separately.

Table C.3 presents the point-in-time and full-period impact estimates. In most cases, the point-in-time estimates were developed by using ordinary least squares regression methods. The exceptions are the two schooling variables and the four living arrangement variables, for which estimates are the difference between the means for the experimental and control groups. Both types of estimation procedures yield unbiased estimates of the impacts of STETS. In all cases, the impacts presented here are consistent with those presented in Chapters IV through VII.

The following are the specific sources of the impact estimates presented in Table C.3:

- Non-STETS earnings: regression estimates using inflation-adjusted data and excluding all STETS earnings (estimates without these adjustments are presented in Table IV.4)
- Income: regression estimates using inflation-adjusted data (unadjusted estimates are presented in Table VII.5)
- Sheltered workshop use: regression estimates are taken from Table IV.4
- Schooling variables (2 variables): experimental-control difference in means (Table V.5 presents regression-based estimates of impacts on the other school variables)
- Non-STETS training: regression estimates using data that excluded STETS training (Table V.5 presents regression-based estimates, including all training)
- Living arrangement variables (4 variables): experimental-control differences in means (Table VII.7 presents similar estimates)
- Receipt of transfers (3 variables): regression estimates (taken from Table VI.3)

TABLE C.3
IMPACT ESTIMATES FOR THE BENEFIT-COST ANALYSIS

Variable	Units	Point-In-Time Estimates 6 Months	 15 Months	 22 Months	Period Estimates Months 1-6	 Months 7-15	 Months 16-22	 Month 22[a]
Non-STETS Earnings	$/wk	-11.64	3.97	9.20	-151.3	-149.6	199.7	39.87
Income	$/wk	20.11	6.89	8.49	261.41	526.46	233.26	36.79
Months of:								
Sheltered workshop	%	-0.043	-0.097	-0.113	-0.189	-0.720	-0.735	-0.113
Regular secondary school	%	-0.067	-0.026	-0.043	-0.201	-0.419	-0.242	-0.043
Other secondary school	%	-0.038	-0.017	0.006	-0.116	-0.248	-0.039	0.006
Non-STETS training program	%	-0.330	-0.135	-0.129	-0.990	-2.093	-0.924	-0.129
Center/institution	%	0.000	-0.006	-0.011	0.000	-0.027	-0.060	-0.011
Group home	%	-0.012	0.009	-0.012	-0.016	-0.014	-0.011	-0.012
Foster home	%	0.004	-0.008	0.006	0.012	-0.018	-0.007	0.006
Semi-independent living program	%	-0.012	0.021	0.022	-0.036	0.041	0.151	0.022
SSI/SSDI	%	-0.047	-0.076	-0.053	-0.141	-0.534	-0.452	-0.053
Other welfare	%	-0.083	-0.023	0.046	-0.240	-0.477	0.081	0.046
Medicaid/Medicare	%	-0.062	-0.054	-0.022	-0.186	-0.522	-0.266	-0.022
Transfer Payments from:								
SSI/SSDI	$/mo	-10.43	-17.53	-17.31	-31.29	-125.82	-121.94	-17.31
Other welfare	$/mo	-7.95	-7.19	9.41	-23.85	-68.13	7.77	9.41

[a] These estimates reflect the estimated impacts for the twenty-second month after randomization. These estimates form the basis for the extrapolation of the estimated impacts.

- Transfer payments (2 variables): regression estimates using inflation-adjusted data (Table VI.3 presents estimates without this adjustment)

Table C.4 illustrates the process used to estimate cumulative impacts for the out-of-program output variable. The top panel of the table indicates how the estimate of total compensation (gross wages plus fringe benefits) was derived from the interview data, which included only net (i.e., after-tax) wages. This process is described in the next section (Section E.1). The bottom panel of Table C.4 summarizes the interpolation process. The first step was to average the point-in-time estimates for the three periods--months 1 to 6, months 7 to 15, and months 16 to 22. These averages were then multiplied by the length of the associated periods to estimate the cumulative impacts. For example, consider the seven-month period from month 15 to month 22. The two point-in-time estimates are $5.76 per week (the impact at month 15) and $13.36 per week (the impact at month 22). Their average is $9.56 per week. This estimate is then multiplied by 30.3 (the number of weeks in the seven-month period) to yield an estimate of the average cumulative impact over that period, $290 per participant. This figure is then discounted to the time of enrollment using a real annual discount rate of 5 percent. The resulting present value for that period is $268 per participant. This procedure was used for all impacts in all time periods to estimate the cumulative impacts presented in Table C.3.

E. ESTIMATED VALUES FOR IMPACTS

The impact estimates presented in Table C.3 form the basis of the benefit-cost analysis. However, these estimates are generally not denominated in dollars and must be valued as part of the benefit-cost analysis. This section presents the procedures for valuing these impacts.

1. Non-STETS Output

The benefit-cost analysis incorporates this impact by valuing the increase in the output produced by participants. This value is estimated by using the earnings-impact estimate as the basis for estimating the

TABLE C.4

ESTIMATES OF CHANGES IN OUT-OF-PROGRAM OUTPUT
(1982 dollars)

Description	Units	Point-In-Time Estimates			
		Baseline	Month 6	Month 15	Month 22
Change in Out-of-Program After-Tax Earnings	$/week	0	-11.64	3.97	9.20
Change in Out-of-Program Pre-Tax Earnings[a]	$/week	0	-14.32	4.88	11.32
Change in Total Out-of-Program Compensation[b]	$/week	0	-16.90	5.76	13.36

Description	Units	Cumulative Estimates			
		Months 1-6	Months 7-15	Months 16-22	Total
Cumulative Change in Out-of-Program Output[c]	$/participant	-220	-217	290	-147
Discounted Value of Cumulative Changes in Out-of-Program Output[d]	$/participant	-217	-208	268	-156

NOTE: Details do not always sum to totals because of rounding.

[a] Pre-tax earnings are estimated as 1.23 times after-tax earnings to reflect the withholding of 18.7 percent of pre-tax earnings, the rate for minimum-wage workers.

[b] Non-wage components of total compensation (the difference between pre-tax earnings and total compensation) are estimated to be 18 percent of pre-tax earnings.

[c] The cumulative changes in total compensation are estimated by linearly interpolating between the point estimates.

[d] A 5 percent real annual rate is used to discount the value of effects to the time of enrollment.

impact on total compensation (i.e., wages plus fringe benefits). Total compensation will equal the value of output produced if markets function competitively. Specifically, if the firms are profit-maximizers and compete in competitive product markets, they will then continue to hire workers to a point at which the contribution made to total output by the last worker hired (i.e., the value of the marginal product of labor) is equal to the compensation rate of the workers. At the same time, if labor markets function competitively, workers can move freely between firms in an attempt to obtain higher wages, so that all firms will have to provide equal compensation rates to workers who exhibit the same skill level. Together, these actions will lead to an equality between the total cost of an employee (i.e., the worker's total compensation) and the value of goods and services produced by that worker. Thus, under the assumption that sample members work in markets that can be characterized as competitive, the value of their output in those markets can be assumed to equal the compensation they receive.[1]

The interview data pertained to after-tax earnings rather than to total compensation. Thus, we had to multiply the impact estimate by an estimate of the average tax-withholding rate and by an estimate of the average fringe-benefit rate, where both rates pertain to workers in jobs such as those held by sample members.

The tax-withholding rate was estimated from Internal Revenue Service regulations. In 1982, the minimum withholding rate for federal income-tax purposes was 12 percent. At the same time, the FICA tax rate for Social Security was 6.7 percent. Thus, the combined rate (ignoring withholdings for state and local taxes) was 18.7 percent of gross wages, or, equivalently, 23 percent of after-tax wages.

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[1] Because sheltered workshops are required to pay compensation that reflects productivity, this argument can reasonably be assumed to apply to sample members in both sheltered workshop and regular jobs. The correspondence between total compensation and the value of output produced is less clear for persons in subsidized training jobs.

The U.S. Department of Labor (1980) estimated that wages and salaries for private nonfarm employees in 1977 accounted for 84.6 percent of total compensation. The remaining 15.4 percent consisted of supplements to wages and salaries, which included employer expenditures for retirement programs (both private retirement and Social Security), life, accident, and health insurance, Workers' Compensation, Unemployment Insurance, and savings and thrift plans. These percentage estimates imply that the fringe-benefit rate for these employees is equal to 18.2 percent of wages and salaries.

Smeeding (1981), providing a more recent estimate of fringe-benefit rates, used microsimulation techniques to estimate the average cost of fringe benefits to employers in 1979 and disaggregated the estimate by the yearly earnings of the employees. His estimates suggest that, in 1979, workers who were earning $10,000 or less received fringe benefits worth approximately 17.9 percent of wages and salaries,[1] as compared with 19.8 percent for all workers. We have used an estimate of 18 percent, which lies between the Smeeding and DOL estimates.

2. Use of Other Programs

STETS-induced changes in the use of other programs were valued by multiplying the estimated impact on the average months of use by the average cost per person month of serving persons in those other programs. We examined four alternative programs: sheltered workshops, secondary-level vocational programs, other school programs, and other job-training programs. We also considered four types of residential programs: institutions or centers, group homes, foster care, and semi-independent living programs. For all these programs, we used published estimates of average costs; when published estimates were unavailable for 1982, we multiplied the available estimate by the associated change in the implicit price deflator for gross national product. Table C.5 summarizes the values and sources used in the analysis.

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[1] Those with the lowest earnings, $2,000 per year or less, received an estimated 15.9 percent, while those with earnings of $7,501 to $10,000 received an estimated 20.4 percent.

TABLE C.5

ESTIMATED AVERAGE MONTHLY COSTS FOR SCHOOL, TRAINING, AND RESIDENTIAL PROGRAMS

Program	Average Cost per Month (1982 dollars)	Data Source for Estimate
Sheltered Workshop[a]	493	U.S. Department of Labor (1977)
Secondary-Level Vocational Education[b]	520	Kakalik et al. (1981) Table 2.5
Other School Programs[c]	290	Dearman and Plisko (1982) Kakalik et al. (1981) Grant and Eiden (1981)
Other Job Training[d]	113	Kakalik et al. (1981)
Center/Institution	2,060	Hauber et al. (1984) Table 23
Group Home[e]	1,239	Hauber et al. (1984) Table 23
Foster Care	490	Hauber et al. (1984) Table 23
Semi-Independent Living Program	830	Hauber et al. (1984) Table 23

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[a] This excludes capital costs. The cost nets out the revenue received from sales of workshop-produced goods and services.

[b] This is a weighted average for serving learning-disabled students and educable and trainable mentally retarded students. The weights are the proportions of borderline, mild, and moderately retarded sample members.

[c] This value is the weighted average of the costs of college, $368/month (Dearman and Plisko, 1982), regular secondary education, $284/month (Kakalik et al., 1981); and postsecondary vocational education, $163/month (Grant and Eiden, 1981). The weights are the proportions of sample members in these types of school at 22 months after randomization.

[d] This value is for a work-study program. The other common type of job-training program for STETS sample members is on-the-job (OJT) training programs. Taggart (1981) estimates that OJT programs funded by CETA cost $121/month (in 1982 dollars).

[e] The group home estimate is a weighted average of the average cost of small group homes (fewer than 16 residents) and larger group homes (16 or more residents). The weights are the proportion of sample members in each facility type.

3. Transfer Programs

Changes in the use of SSI/SSDI, other welfare programs, and Medicaid/Medicare enter the benefit-cost analysis in two ways. Reductions in the amount of transfer payments will be viewed as a cost by participants and as a benefit by taxpayers. The value of these transfers will cancel out from the social perspective and, hence, will not enter the social benefit-cost calculation. Reductions in program administrative costs--the amount of resources devoted to making the transfers--will appear as a benefit to taxpayers. Because an actual resource savings occurs, the administrative cost savings will enter the social calculation.

We estimated the value of changes in payments from SSI/SSDI and other welfare directly from interview data, after adjusting for inflation. We estimated the average monthly administrative cost for SSI/SSDI, $15 in 1982 dollars, by using information in the Budget of the United States Government (1982). The monthly administrative costs for other welfare programs were assumed to equal the administrative costs per recipient for the Aid to Families with Dependent Children (AFDC) program. This cost was approximately $13 per month in 1982 dollars (Budget of the United States Government, 1982).

For Medicaid/Medicare, the average administrative cost per month of eligibility was estimated as $13, the average monthly Medicaid administrative expenditure per recipient (Budget of the United States Government, 1982). The value of Medicaid coverage was estimated as $250, the average Medicaid expenditure per month (in 1982 dollars) for persons who are disabled (Sawyer et al., 1983, Tables 4.4 and 4.15).[1]

The use of average costs to value the change in administrative costs probably overstates short-run effects. In the short-run, the changes in costs should be estimated by marginal rather than by average costs. Marginal costs (the change in total costs due to a change of one recipient) are probably quite low for transfer programs, because staffing and facilities decisions are probably unaffected by small changes in caseloads. However, in the long-run, these decisions are more easily affected by caseload changes, so that average costs may better approximate long-run marginal costs.

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[1] If all Medicaid recipients were included, the average monthly expenditure would be only $105.

4. Tax Payments

Participants' incomes will change as their earnings and transfer payment receipts change. These changes will cause changes in tax payments, including changes in income, payroll, sales, and excise taxes. Changes in the payments of these taxes were estimated by multiplying the average estimated change in participants' incomes by an estimate of the tax rate applicable to total income for low-income households, as obtained by Pechman (1985). His estimates suggest that persons in low-income households (i.e., households in the bottom 20 percent of the income distribution) face an average tax rate of 23 percent.[1]

In interpreting this rate, we should note that for households at the low end of the income distribution a major form of taxation is sales and excise taxes. Because these taxes are based on consumption and sales collected by retail firms, it is very difficult to avoid paying them.[2] The other major form of taxation for these households is the payroll tax, which, when combined with sales and excise taxes, accounts for 80 percent of the total tax burden of low-income households. Thus, even though these households face low rates for the individual income tax, their total tax burden as a percentage of income is not subtantially different from the tax burden of most taxpayers. It should also be noted that Pechman's estimates correct for the general level of income underreporting for tax purposes. He also estimates the incidence of taxes for which the statutory and ultimate tax burdens differ substantially (e.g., property and corporate income taxes, which may be shifted to workers and consumers).

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[1] Pechman presents tax rates for eight sets of incidence assumptions. In determining the 23 percent rate used here, we dropped the alternatives that created the highest (28.9 percent) and lowest (20.6 percent) tax rates and averaged the remaining six.

[2] However, it could be argued that the incidence is at least partially incurred by producers.

Once the rate was estimated, we calculated total income. We defined income in the same manner as in Chapter VII, with the exception that we added estimated tax withholding and fringe benefits to participants' after-tax earnings when estimating the earnings component of total income.

F. SENSITIVITY TESTS

It is evident from this discussion that numerous assumptions are involved in estimating net present value. Thus, a critical component of the benefit-cost analysis is to assess the importance of these various assumptions. We do so by examining the effect on the basic net present value estimate of changing sets of underlying assumptions while keeping all other assumptions unchanged. The pattern of estimates that are derived from these sensitivity tests indicates the overall level of uncertainty in the results. If the estimates change substantially and suggest different qualitative conclusions, then little confidence can be placed in the results. Alternatively, if the qualitative conclusions remain the same under a plausible range of alternative assumptions, then more faith in the conclusions is warranted. In addition, the sensitivity tests highlight those underlying assumptions that are particularly important for the results. Changing some assumptions may cause virtually no change in estimated net present value, while changing others may cause substantial changes. The sensitivity tests can provide a sense of which assumptions and impact estimates are particularly important--a sense that is useful in interpreting the overall findings.

Table C.6 presents alternative estimates of net present value under a variety of different assumptions about the value of impacts and costs.[1]

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[1] Another concern is the precision with which STETS impacts are estimated. In this regard, it should be noted that the earnings effects and the effects on the use of alternative programs (the major benefits) are statistically significant, and the estimates have relatively small standard errors. Moreover, all estimates used are unbiased measures of program impacts. Thus, we feel confident that the impact estimates used here accurately reflect the effects of STETS.

TABLE C.6

ALTERNATIVE ESTIMATES OF NET PRESENT VALUE PER PARTICIPANT
(1982 dollars)

	Estimated Net Present Value		
Valuation Assumptions[a]	Social	Participant	Nonparticipant
Basic Assumptions[b]	$-1,038	$2,111	$-3,149
Extrapolation Beyond 22-Month Observation Period			
7 months; no decay of impacts	4	2,333	-2,329
30 months; no decay of impacts	3,221	3,018	203
10 years; 14 percent annual decay	5,237	3,447	1,791
Value of Phase 1 and Phase 2 Output			
Value = revenue	-3,280	2,111	-5,391
Value = alternative supplier's price	-22	2,111	-2,133
Operational Cost Equals the Cost Observed for the Entire Demonstration	-3,552	1,555	-5,107
Discount Rate			
3 percent	-1,025	2,131	-3,157
10 percent	-1,068	2,060	-3,128
Indirect Labor Market Effects	-2,677	2,111	-4,788

[a] A full description of each set of evaluation assumptions is provided in the text.

[b] See Table VIII.5 for details on the value of specific benefits and costs under the basic assumptions.

The specific estimated values change substantially, but the set of estimates seems to indicate a consistent conclusion--that STETS will be an economically efficient investment if the impacts that are observed 22 months after randomization persist for as little as seven months, and if program operating costs can be held at the levels observed during the demonstration steady-state period.

The assumptions used in the basic estimate are those that we emphasized in Chapter VIII and in this appendix: that no impacts occur beyond the 22-month observation period; that the value of Phase 1 and Phase 2 output is the net value added observed in the case studies; that research costs and the extraordinary operating costs did not affect program impacts; that the real (i.e., net of inflation) discount rate is 5 percent per year; that no indirect labor-market effects occur; and that the interpolations between observed impacts are accurate (in addition, a number of other assumptions are less central to the analysis). Under these assumptions, we estimate that social benefits are approximately $1,000 per participant less than social costs. Substantial benefits occur for workshops and other alternative programs. Nonetheless, these benefits are not large enough to offset the program costs under the basic assumptions.

This basic result almost certainly underestimates the true net present value, because at least some impacts should persist beyond the 22-month observation period. In particular, the impacts on earnings and alternative program use should continue for awhile. These impacts are relatively large and statistically significant. Furthermore, these impacts appear to be relatively stable between months 15 and 22. Thus, the basic net present value estimate probably undercounts the benefits associated with these impacts.

While it seems clear that these benefits will persist, it is impossible to determine how long they will continue or whether the impacts will increase or decrease over time. It may be that the STETS model, which provides transitional rather than long-run services, generates impacts that decline shortly after the end of program services. Alternatively, some participants may have successfully made the transition into the competitive labor market and will continue to earn more than they would have in the absence of STETS. In the absence of information on long-run effects, we excluded all future impacts from the basic benefit-cost estimate and examined potential future impacts in sensitivity tests.

Table C.6 presents three sensitivity tests pertaining to impacts beyond the observation period. These alternative estimates, along with the basic estimates, suggest the plausible range of benefits. If the impacts that are observed at 22 months after randomization persisted at that level, social net present value would be positive in seven months, and nonparticipants would perceive a net benefit within 2.5 years. If we adopted a ten-year planning horizon and assumed that all impacts declined by one-half every five years (a 14 percent annual decay rate), then we would find that STETS would generate substantial net benefits from all three perspectives. All of these alternative assumptions are arbitrary. Nonetheless, they suggest that, as long as the impacts do not decay very quickly (and we have no evidence to suggest that they will), then STETS is likely to generate benefits that exceed costs from the perspective of society and participants. The issue is less clear for nonparticipants--STETS will need to produce relatively long-run impacts in order to generate a net economic benefit to nonparticipants.

The estimates in Table C.6 also show that the assumptions made about the value of Phase 1 and Phase 2 output have a large affect on the overall results. The social and nonparticipant net present value estimates change by over $3,200, depending on whether this output is assigned the lower-bound estimate (service project revenue) or a likely upper-bound estimate (gross alternative supplier's price). However, this range probably overstates the uncertainty surrounding the value of this output. As stated earlier, revenue almost certainly understates the actual value of the output because the demonstration projects focused on generating jobs rather than revenues. Moreover, the alternative supplier's price is probably too high because there were several instances in which participant labor did not lead to net increases in output. What these estimates do indicate is the potentially important role of Phase 1 and Phase 2 output. clearly, placing participants in productive jobs as quickly as possible can increase net benefits.

The next sensitivity test in Table C.6 shows the affect of including the extra operational costs incurred because STETS was a demonstration. This estimate excludes estimated research costs, but includes all other project-level costs for operations and participant compensation. It indicates that STETS would be much less likely to be economically efficient if costs reached this level (almost $11,000 per participant rather than $9,400 estimated for the steady state). It is unlikely that all the extra costs associated with start-up and the relatively small scale of operations affected impacts. However, some uncertainty surrounds the actual costs. It will be crucial to monitor costs closely in any future STETS-type efforts and to try to keep them at the steady-state levels. This situation seems plausible given that (1) projects were able to operate at these cost levels for five months and that (2) the impacts estimated for persons who were served during the steady-state period seem to be larger than those estimated for participants who were enrolled and received the bulk of their services at other times (see Table IV.7). Therefore, we feel that steady-state costs are the most appropriate costs for judging the economic efficiency of STETS.

The discount-rate sensitivity tests show that the choice of a discount rate is not crucial to this evaluation. Increasing the discount rate to 10 percent a year reduces net present value by 2.9 percent ($30). Lowering it increases net present value by 1.3 percent. These changes are trivial within the scope of the evaluation.

The last sensitivity test in Table C.6 indicates the effects of assuming that half of the work performed by participants would have been performed by other workers in the absence of STETS. It also assumes that half of the work foregone by participants when they entered STETS is performed by other workers. These alternative assumptions, which are discussed in Chapter VIII, affect only those benefits that are associated with participant output. Thus, even though they would substantially reduce the value of net present value during the observation period, it is still plausible that STETS would generate sufficient savings from the reduced use of other programs to pay for itself from the social perspective.

Other MDRC Studies
on the STETS Demonstration

Supported Work for the Mentally Retarded: Launching the STETS Demonstration. Bangser, Michael; and Price, Marilyn. 1982.

A Transitional Employment Strategy for the Mentally Retarded: The Final STETS Implementation Report. Riccio, James; with Price, Marilyn. 1984.

Lessons on Transitional Employment: The STETS Demonstration for Mentally Retarded Workers. Bangser, Michael. (A monograph summarizing the research findings and operational experience.) 1985.

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